746

PROCEEDINGS

OF THE

PHYSICAL SOCIETY OF LONDON

From December 1924 to August 1925

VOL. XXXVII.

LONDON:
FLEETWAY PRESS LTD.,
3-9, DANE STREET, HIGH HOLBORN, W.C.1.
MDCCCCXXV.

10633

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DANE STREET, HOLBORN, W.C.1

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PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY OF LONDON

SESSION 1924-1925.

Except where otherwise stated, the meetings were held at the Imperial College of Science, the President being in the chair.

October 24, 1924.

The President announced that owing to a breakdown in health due to overwork Mr. COOPER, the Society's Treasurer, had felt compelled to resign. The Council had accepted the resignation with great regret, and had appointed Mr. R. S. WHIPPLE as Acting Treasurer. The Council proposed to send to Mr. COOPER a letter expressing sincere thanks for his past services to the Society and hopes for his speedy recovery to complete health. With this resolution the Fellows present unanimously associated themselves.

The President also announced that during the vacation Mr. A. V. PARKE, the Society's attendant, had died. It was resolved that a message of sympathy be sent to his widow.

1. "Underblown Pipes," by D. GUNNAIYA, M.A., and G. SUBRAHMANIAM, M.A., with Demonstration by D. J. BLAIKLEY.

2. "On the Temperature Gradient in Gases at Various Pressures," by W. MANDELL, B.Sc., and J. WEST, B.Sc.

3. "Vectorial Dimensions," by J. F. S. ROSS, M.C., B.Sc.

November 14, 1924.

A DEMONSTRATION of an "Electrical Method of Synthesising Vowel Sounds" was given by Dr. W. H. ECCLES, F.R.S.

The following Papers were read :—

1. "The Dielectric Properties of Mica," by D. W. DYE, B.Sc., and L. HARTSHORN, B.Sc., D.I.C., The National Physical Laboratory.

2. "The X-Ray Emission of Electrons from Metal Films, with Special Reference to the Region of the Absorption Limit," by Dr. LEWIS SIMONS, Birkbeck College, London.

November 28, 1924.

A **Discussion**, arranged jointly with the Royal Meteorological Society, was held on the subject of "Ionization in the Atmosphere, and its Influence on the Propagation of Wireless Signals." Chairman : F. E. **Smith**, C.B.E., F.R.S.

PROGRAMME.

1. Opening Address, by Dr. W. H. **Eccles**, F.R.S.

2. "Atmospheric Ionization and its Variations," by Dr. C. **Chree**, F.R.S.

3. "Experimental Observations of the Strength of Wireless Signals as affected by the Path of the Signals, Time of Day and Season of Year," by Prof. E. V. **Appleton**, M.A., D.Sc.

4. "Atmospherics," by R. A. **Watson-Watt**.

5. "The Electric Field of a Thunderstorm and Some of its Effects," by C. T. R. **Wilson**, F.R.S.

6. "The Evidence of Terrestrial Magnetism for the Existence of Highly Ionized Regions in the Upper Atmosphere," by Prof. **Sydney Chapman**, F.R.S.

December 12, 1924.

The following Papers were read :—

1. "Numerical Values of Chemical Constants and Frequencies of the Elements," by Capt. A. C. **Egerton**.

2. "The Sensibility of Circular Diaphragms for the Reception of Sounds in Water," by J. H. **Powell**, M.Sc.

3. "A Direct-reading Frequency Meter of Long Range," by **Albert Campbell**, B.A.

A **Demonstration** of the Frequency Meter was given by W. H. **Lawes**, of the Cambridge Instrument Co.

January 7 and 8, 1925.

The Annual Exhibition of Apparatus was held by the Physical Society of London and the Optical Society from 3-6 p.m. and from 7-10 p.m. each day.

Discourses were given as follows :—

On January 7 at 4 p.m. and January 8 at 8 p.m., "Some Experiments with Interferometers," by F. **Twyman**, F.R.S.

On January 7 at 8 p.m. and January 8 at 4 p.m., "Talking Motion Pictures," by C. F. ELWELL, B.A., M.I.E.E.

January 23, 1925.

The following Papers were read :—

1. "An Investigation into Corrections Involved in the Measurement of Small Differences in Refractive Index of Dispersive Media by means of the Rayleigh Interferometer, with Special Reference to the Application of the Results to Measurements in Diffusion," by B. W. CLACK, Ph.D., Birkbeck College, London.

2. "A Study of the Production of Flashing in Air Electric Discharge Tubes," by J. TAYLOR, B.Sc., and W. CLARKSON, B.Sc., Armstrong College, Newcastle-on-Tyne.

A DEMONSTRATION entitled "A Kinematographic Study of the Formation of Plateau's Spherule" was given by C. R. DARLING, F.I.C.

February 13, 1925.

Annual General Meeting.

GENERAL BUSINESS.

The Report of the Council and that of the Treasurer were presented and unanimously adopted.

REPORT OF THE COUNCIL.

The chief event of the year has been the celebration of the Jubilee of the Society by various meetings held on March 20 and 21, and a Banquet on March 22. There were large attendances at all these functions, and other Societies were well represented by delegates. At the Jubilee Banquet the Society was honoured by the presence of H.R.H. the Duke of York and the Prime Minister (Mr. J. Ramsay MacDonald). The Society received numerous addresses and messages of congratulation. Details of the celebrations have been published in a special number of the Proceedings.

Eleven ordinary Science Meetings have been held during the year, ten at the Imperial College of Science, and one, on June 27, by kind invitation of Prof. O. W. Richardson, at King's College. In addition, by kind invitation of Sir Ernest Rutherford and the Cambridge Instrument Company, the Society paid on July 19 a visit to Cambridge, where a Science Meeting was held in the Cavendish Laboratory. The visitors spent the morning in the works of the Cambridge Instrument Company, and were entertained to lunch in Hall of Trinity College, by the Directors of the Company.

At the Science Meetings 36 Papers were presented and 13 Demonstrations were given.

On February 14 there was a continuation of the Joint Discussion with the Institution of Electrical Engineers (commenced on November 29, 1923) on "Loud-Speakers for Wireless and Other Purposes." This meeting was held at the Institution and was largely attended.

The Society also co-operated with the Royal Meteorological Society in a Joint Discussion on "Ionization in the Atmosphere and its Influence on the Propagation of Wireless Signals," held at the Imperial College on November 28. Dr. W. H. Eccles opened the Discussion; the attendance numbered about 170.

The Ninth Guthrie Lecture formed part of the Jubilee celebrations at the Institution of Electrical Engineers on March 20. It was delivered by M. le Duc de Broglie on "The Photo-electric Effect in the case of High Frequency Radiation, and some Associated Phenomena." About 250 Fellows and visitors were present.

On January 25 Mr. E. A. Milne gave a lecture to the Society on "Recent Work on Stellar Physics."

Apart from the Joint Discussions and the Guthrie Lecture the average attendance at the meetings of the Society was 66.

The Fourteenth Annual Exhibition of Scientific Apparatus, arranged jointly by the Physical and Optical Societies, was held, through the courtesy of the Governing Body, at the Imperial College on January 2 and 3. Fifty-five firms took part in the Exhibition, which was, as usual, well attended. Discourses were given by Mr. H. B. Grylls on "The Heape and Grylls Rapid Cinema Machine," and by Sir Richard Paget, Bart., on "The Nature and Artificial Production of Human Speech."

At the Centenary celebrations of the Franklin Institute, Philadelphia, in September, Sir William Bragg represented the Society as official delegate and presented on behalf of the Society an Address of Congratulation in the following terms:—

> The Physical Society of London sends greetings to the Franklin Institute on the happy occasion of the hundredth anniversary of its foundation. By the people of Great Britain the name of Franklin has been held in high esteem, both in his life-time and as a cherished memory. Your Institute during the past hundred years has done much to maintain interest in the broadest aspects of physical science and has thus acted as a monument to the versatility and genius of the man in whose honour the foundation was made. That this most useful work may continue and flourish is the sincere wish of the Physical Society of London.
>
> On behalf of the Society—
>
> F. E. SMITH, *President.*
>
> W. R. COOPER, *Treasurer.*
>
> ARTHUR SCHUSTER, *Foreign Secretary.*
>
> DAVID OWEN
> A. O. RANKINE } *Secretaries.*

The President, Mr. F. E. Smith, and Prof. A. O. Rankine were appointed as delegates to the International Mathematical Congress in Toronto in August.

Mr. F. E. Smith and Dr. D. Owen have been appointed representatives of the Society on the Board of the Institute of Physics, and Mr. T. Smith and Dr. D. Owen on the Science Abstracts Committee. Mr. J. Guild has been appointed, in succession to Mr. T. Smith, on the National Committee for Physics, and Dr. J. Robinson, in succession to Mr. F. E. Smith, on the National Committee for Radio-telegraphy.

At the request of the British Academy Tokyo Committee the Council has presented to the Tokyo University Library Vols. 1 to 27 of the Proceedings to replace losses incurred in the 1923 earthquake.

The Council has invited Prof. A. Fowler, F.R.S., to write a Supplement to his Report on Series in Line Spectra. Prof. Fowler has agreed to do so, and the Supplement, which will reach considerable dimensions owing to the rapid growth of the subject, will be published early in 1925.

The Council has authorised the issue of the Society's Special Reports to Students of the Society at half the published price.

In October, owing to a breakdown in health, Mr. W. R. Cooper, the Treasurer of the Society, was compelled to resign. The Council has accepted the resignation with great regret, and the Society has expressed its sympathy with Mr. Cooper and sincere thanks for his great services to the Society. Mr. R. S. Whipple was appointed Acting Treasurer and co-opted on the Council.

Mr. A. V. Parke, one of the Society's attendants, died suddenly in July. The Society has expressed deep sympathy with the relatives and the Council has subscribed £25 to the fund in aid of the widow.

The Council has awarded the Second Duddell Memorial Medal to Prof. C. Vernon Boys, F.R.S. This Medal will be presented at the Annual General Meeting.

The Society has to record with regret the deaths of Prof. J. D. van der Waals, Prof. G. H. Quincke, Prof. F. D. Brown, Mr. F. Jacob, Mr. J. Grundy, Mr. Chichester A. Bell, Prof. Julius Wertheimer, Dr. R. M. Walmsley and Mr. J. St. V. Pletts. Prof. van der Waals, whose death occurred in 1923 (reference being inadvertently omitted in the 1923 Report), was an Honorary Fellow elected in 1892. So also was Prof. Quincke, the date of his election being 1882. Prof. Brown was a Life Fellow elected very shortly after the foundation of the Society in 1874. Mr. Jacob and Mr. Grundy were also Life Fellows, elected in 1879 and 1884 respectively. Mr. Chichester Bell became a Fellow in 1886. Prof. Wertheimer, Dr. Walmsley and Mr. St. V. Pletts were all Life Fellows, having been elected in 1886, 1888 and 1919 respectively.

The number of Honorary Fellows on the Roll on December 31, 1924, remained at ten, Prof. Max Planck having been elected an Honorary Fellow at the last Annual General Meeting. The number of Ordinary Fellows and Students was 646, this being considerably greater than at the end of 1923. During the year 62 New Fellows and 5 Students have been elected, and there were 10 resignations.

INCOME AND EXPENDITURE ACCOUNT.

From January 1st to December 31st, 1924.

Expenditure.	£	s.	d.	£	s.	d.
"Science Abstracts" (Inst. El. Eng.)				281	0	0
Ordinary Publications :—						
"Proceedings"	786	14	2			
"Bulletin"	63	11	5			
General	102	14	2			
				952	19	9
Postage of Publications				87	6	2
Reporting	61	14	4			
Refreshments and Expenses of Ordinary Meetings	26	19	4			
Expenses of Exhibition held jointly with the Optical Society	181	14	0			
Secretary's Expenses	28	8	10			
Treasurer's Expenses	18	14	1			
Periodicals	7	15	0			
Insurance	1	15	0			
Registered Address	2	2	0			
Bank Charges and Cheque Books	0	10	6			
Advertising	4	12	0			
Sundry Expenses	15	12	0			
				349	17	1
Guthrie Lecture (Honorarium)	10	0	0			
Donation to the Institute of Physics towards the Scientific Instrument Journal Guarantee Fund	20	0	0			
Grant to the Widow of A. V. Parke	25	0	0			
				55	0	0
Excess of Income over Expenditure carried down				277	12	8
				£2,003	15	8
Jubilee Celebrations :—						
Expenses at Meetings, Exhibition and Banquet	£293	19	2			
Special Number of the Proceedings	149	13	6			
				£443	12	8

Income.	£	s.	d.	£	s.	d.
Entrance Fees				46	4	0
Subscriptions by Fellows * (including £70 15 5 Arrears due 1924)	866	5	8			
Subscriptions due from Institute of Physics	10	10	0			
,, Voluntary	9	9	0			
,, by Students	8	18	6			
,, Arrears paid £46 12 9 ,, Less: Reserved in 1923 42 0 3	4	12	6			
,, for "Science Abstracts" and Advance Proofs	36	0	0			
				935	15	8
Sales of Publications (Fleetway Press, Ltd.)				639	17	7
Dividends :—						
London Midland & Scottish Railway	43	7	4			
Metropolitan Board of Works	7	0	0			
Lancaster Corporation Stock	9	6	0			
New South Wales Stock	14	4	11			
Southern Railway	23	9	5			
London & North Eastern Railway	15	10	0			
India 3½% Stock	17	10	0			
Funding Loan	20	3	0			
War Loan 5%	35	0	0			
	185	10	8			
Less: Transferred to Duddell Memorial Trust Fund	20	0	0			
				165	10	8
Income Tax repayable for two years to 31st December, 1924				58	17	3
Fee for the French rights for translating Dr. Jean's report on Radiation				5	0	0
Interest on deposit account				5	5	4
Optical Society for share of Exhibition expenses				8	6	8
Exhibitors' payments on account of Exhibition Catalogue				138	18	6
				£2,003	15	8
Excess of Income over Expenditure brought down	£277	12	8			
Balance carried to Balance Sheet	166	0	0			
				£443	12	8

W. R. COOPER, *Honorary Treasurer.*
ROBERT S. WHIPPLE, *Acting Honorary Treasurer.*

Audited and found correct,
D. DYE
R. L. SMITH-ROSE } *Honorary Auditors.*

February 6th, 1925.

*** Ninety-four Fellows paid reduced subscriptions by the arrangement with the Institute of Physics, the total discount amounting to £20 2s. 1d. "Voluntary Subscriptions" are subscriptions paid by Fellows who compounded for the low sum of £10.**

BALANCE SHEET AT DECEMBER 31ST, 1924.

LIABILITIES.			£ s. d.	£ s. d.
Accumulated Fund:—				
Balance as per last account			1,941 5 4	
Add:—				
Increased value of Investments		£140 0 0		
Less Duddell Memorial Trust Fund	£6 0 0			
Less W. F. Stanley Trust Fund	29 0 0			
		35 0 0		
			105 0 0	
			2,046 5 4	
Less Excess of Expenditure over Income			166 0 0	
				1,880 5 4
Life Compositions:—				
Balance as per last Account			2,038 10 0	
Added during year			115 10 0	
				2,154 0 0
Duddell Memorial Trust Fund				432 12 9
W. F. Stanley Trust Fund				449 0 0
Sundry Creditors				149 13 3
Subscriptions paid in advance				41 13 4
				£5,107 4 8

ASSETS.	£ s. d.	£ s d
Investments at Market Value on date:—		
£399 London Midland & Scottish Railway 4 per cent. Debenture Stock	337 0 0	
£1,000 London Midland and Scottish Railway 4 per cent. Preference Stock	801 0 0	
£200 Metropolitan Board of Works 3½ per cent. Consolidated Stock	189 0 0	
£400 Lancaster Corporation 3 per cent. Redeemable Stock	293 0 0	
£254 2s. 9d. New South Wales 5 per cent. Inscribed Stock, 1935-55	257 0 0	
£300 Southern Railway Preferred Ordinary Stock	253 0 0	
£442 Southern Railway Deferred Ordinary Stock	196 0 0	
£500 London & Eastern Railway 4 per cent. Debenture Stock	417 0 0	
£500 India 3½ per cent. Stock	335 0 0	
£650 4% Funding Loan, 1960-90	580 0 0	
£300 5% War Loan 1929/47 inscribed, "A" Account	304 0 0	
£400 5% War Loan 1929/47 inscribed, "B" Account (Duddell Memorial Fund)	406 0 0	
		4,368 0 0
Stock of Publications (as per Treasurer's valuation)		400 0 0
Debtors—		
Due from Institute of Physics	10 10 0	
,, ,, Optical Society	8 6 8	
,, ,, Arrears of Subscriptions	70 15 5	
,, ,, Customs and Excise for Income Tax reclaim	58 17 3	
		148 9 4
Cash at Bankers	189 7 9	
Cash in hand of Treasurer	1 7 7	
		190 15 4
		£5,107 4 8

W. R. COOPER, *Honorary Treasurer.*
ROBERT S. WHIPPLE, *Acting Honorary Treasurer.*

Audited and found correct,

D. DYE
R. L. SMITH-ROSE } *Honorary Auditors.*

February 6th, 1925.

Note.—**The value of the Society's library has not been brought into the Balance Sheet.**

LIFE COMPOSITION FUND AT DECEMBER 31ST, 1924.

	£	s.	d.
128 Fellows paid £10	1,280	0	0
3 Fellows paid £15	45	0	0
1 Fellow paid £20 10s.	20	10	0
13 Fellows paid £21	273	0	0
17 Fellows paid £31 10s.	535	10	0
162	£2,154	0	0

W. F. STANLEY TRUST FUND (FOR THE "BULLETIN").

	£	s.	d.		£	s.	d.
£300 Southern Railway Preferred Ordinary Stock	253	0	0	Carried to Balance Sheet	449	0	0
£442 Southern Railway Deferred Ordinary Stock	196	0	0				
	£449	0	0		£449	0	0

DUDDELL MEMORIAL TRUST FUND.

CAPITAL.

	£	s.	d.		£	s.	d.
£400 War Loan 5% 1929/47 inscribed Stock	406	0	0	Carried to Balance Sheet	406	0	0

REVENUE.

	£	s.	d.		£	s.	d.
Balance at 31st December, 1923	26	12	9	Honorarium to Medallist	20	0	0
Dividends	20	0	0	Balance carried to Balance Sheet	26	12	9
	£46	12	9		£46	12	9

Audited and found correct,

W. R. COOPER, *Honorary Treasurer.*
ROBERT S. WHIPPLE, *Acting Honorary Treasurer.*

February 6th, 1925.

D. DYE
R. L. SMITH-ROSE } *Honorary Auditors.*

REPORT OF THE TREASURER.

It has been considered advisable to remodel the method of presenting the accounts so that the financial position of the Society can be more easily ascertained. The Life Composition Fees have been credited to the Life Composition Fund shown on the liability side of the balance-sheet, and not credited to the income and expenditure account, as in previous years. In a similar manner the subscriptions paid in advance have been deleted from the income and expenditure account, and are included as assets in the balance-sheet. The accounts show that the expenditure for the year exceeded the income by £166. An expenditure of £443 12s. 8d. was incurred in connexion with the Jubilee Celebrations of the Society. If this amount is deducted from the expenditure, the excess of income over expenditure amounts to £277 12s. 8d.

Fellows of the Society will be gratified at the increase in the amount received from the sale of the publications of the Society, the sum so received this year amounting to £639 17s. 7d., an increase of £140 9s. 8d. over that received last year.

The investments have been valued at market prices through the courtesy of the Manager of the Charing Cross Branch of the Westminster Bank . owing to the rise in value ot the securities, the market value of the Society's investments has increased during the twelve months by £140.

ELECTION OF OFFICERS AND COUNCIL.

Dr. Lewis Simons and Dr. B. W. Clack having been appointed Scrutators, a ballot was held for the Officers and Council for the ensuing year. The following were elected :—

President.—F. E. Smith, C.B.E., F.R.S.

Vice-Presidents (who have filled the office of President).—Sir Oliver J. Lodge, D.Sc., F.R.S., Sir Richard Glazebrook, K.C.B., D.Sc., F.R.S., C. Chree, Sc.D., LL.D., F.R.S., Prof. H. L. Callendar, M.A., LL.D., F.R.S., Sir Arthur Schuster, Ph.D., Sc.D., F.R.S., Sir J. J. Thomson, O.M., D.Sc., F.R.S., Prof. C. Vernon Boys, F.R.S., Prof. C. H. Lees, D.Sc., F.R.S., Prof. Sir W. H. Bragg, K.B.E., M.A., F.R.S., Alexander Russell, M.A., D.Sc., F.R.S.

Vice-Presidents.—E. H. Rayner, M.A., Sc.D., J. H. Vincent, D.Sc., M.A., D. Owen, B.A., D.Sc., C. R. Darling, F.I.C.

Secretaries.—Prof. A. O. Rankine, O.B.E., D.Sc., Imperial College of Science and Technology ; J. Guild, A.R.C.Sc., D.I.C., National Physical Laboratory, Teddington, Middlesex.

Foreign Secretary.—Sir Arthur Schuster, Ph.D., Sc.D., F.R.S.

Treasurer.—R. S. Whipple, 45, Grosvenor Place, S.W.1.

Librarian.—J. H. Brinkworth, M.Sc., A.R.C.Sc., Imperial College of Science and Technology.

Other Members of Council.—R. W. Paul, Prof. A. M. Tyndall, D.Sc., W. S. Tucker, D.Sc., J. S. G. Thomas, D.Sc., D. W. Dye, B.Sc., Prof. F. L. Hopwood, D.Sc., E. A. Owen, B.A., D.Sc., J. Robinson, M.Sc., Ph.D., G. B. Bryan, D.Sc.

ELECTION OF AN HONORARY FELLOW.

PROFESSOR W. WIEN was elected an Honorary Fellow of the Society.

DEATH OF A DISTINGUISHED FELLOW.

The President announced the death of Mr. Oliver Heaviside, F.R.S. At his suggestion the Fellows present signified, by standing, their regret and their sympathy with Mr. Heaviside's relatives.

VOTES OF THANKS.

The following votes of thanks were moved: (*a*) To the Auditors (Messrs. D. Dye and R. L. Smith-Rose), moved by Dr. J. S. G. Thomas and seconded by Dr. E. H. Rayner. (*b*) To the retiring Officers and Council (special mention being made of the valuable services of Dr. D. Owen, who retires from the secretaryship), moved by Dr. C. V. Drysdale, O.B.E., and seconded by Mr. W. C. S. Phillips. (*c*) To the Governors of the Imperial College of Science, moved by Mr. T. Smith and seconded by J. Guild.

ORDINARY MEETING FOLLOWING THE ANNUAL GENERAL MEETING.

The **PRESIDENT**, Mr. F. E. Smith, F.R.S., delivered an address entitled "A System of Electrical Measurements." The address was illustrated by experiments.

February 27, 1925.

PRESENTATION OF THE DUDDELL MEDAL.

The **PRESIDENT** presented the Duddell Medal to Prof. C. V. Boys, F.R.S., and gave the following account of Prof. Boys' work:—

Prof. Boys is one of the oldest Fellows of the Physical Society, and in the early history of the Society he took a most active part. He has been Demonstrator, Secretary and President of this Society. The Duddell medal is essentially an award for producing apparatus to advance science, and Prof. Boys is renowned not only for his knowledge of physics, but for his exceptional constructive ability. He invented the first method for producing quartz fibres; indeed, it was he who first had the idea of making fibres of very small diameter and freedom of elastic fatigue; he made a most beautiful torsion balance for measuring the Constant of Gravitation; he invented the radio-micrometer—that beautifully sensitive instrument which will measure the heat radiated from a candle at a distance of three miles; he it was who first photographed a bullet in flight, and afterwards showed us how to blow soap bubbles and study the thin films of which they are formed. A little more than a year ago he exhibited a most beautiful calorimeter, and in the future I feel sure he will produce many other beautiful and valuable instruments. All his apparatus is characterized by exceptional beauty of design—as an experimenter he is unexcelled. It is with the greatest pleasure that I make on behalf of the Society the presentation of the Duddell Medal to Prof. Boys.

Prof. **BOYS**, after expressing appreciation of the award, made some remarks as to the ideals he had kept before him in the design of apparatus. It was comparatively easy to increase the sensibility of instruments, if one had no objection to multiplying the experimental errors by a corresponding factor, but he had always made it his aim to attain the highest sensibility while keeping down such errors. Unless such precautions were taken the strangest phenomena might be observed with highly sensitive apparatus, in consequence, for instance, of convection current disturbances.

The following Papers were read :—

1. "Notes Concerning the Sprengel Pump," by J. J. MANLEY, M.A., Research Fellow of the Magdalen College, Oxford.

2. "The Thomson Effect in Copper, Iron, and Carbon Steels," by J. YOUNG, B.Sc., A.R.C.Sc., F.R.A.S., Lecturer in Physics at the University of Birmingham.

3. "An Improved Cathode-Ray Tube Method for the Harmonic Comparison of Frequencies and for the Delineation of their Wave Form," by D. W. DYE, B.Sc., of the National Physical Laboratory.

A DEMONSTRATION was given in illustration of Paper No. 3.

March 13, 1925.

The following Papers were read :—

1. "The Variation of Young's Modulus at High Temperatures," by JAS. P. ANDREWS, B.Sc., East London College.

2. "The Critical Velocity of Flow Past Objects of Aerofoil Section," by E. G. RICHARDSON, B.A., M.Sc., Ph.D., University College, London.

3. "A Focussing Method of Crystal Powder Analysis by X-Rays," by J. BRENTANO, D.Sc., Lecturer in Physics, Manchester University.

March 27, 1925.

The following Papers were read :—

1. "Electrical Conditions Arising at a Gas-Liquid Interface," by H. W. GILBERT, B.Sc., and P. E. SHAW, B.A., D.Sc.

2. "A Contact Theory of Dielectric Absorption and Power Loss," by L. HARTSHORN, A.R.C.Sc.

The following DEMONSTRATIONS were given by Mr. NORMAN KIPPING, of the International Western Electric Company :—

1. "The Use of the Neon Lamp for Producing an Even Time Base for the Delineation of Wave Forms."

2. "An Examination of Some Dynamic Characteristics of Valves."

3. "A Method of Determining the Percentage Modulation and Reproduction Characteristics of a Radio Transmitting Equipment."

April 24, 1925.

The Tenth Guthrie Lecture was delivered by **PROF. W. WIEN**, who took as his subject "Recent Researches in Positive Rays."

A vote of thanks to the Lecturer was proposed by Dr. F. W. **ASTON**, F.R.S., seconded by Dr. W. H. **ECCLES**, F.R.S., and carried by acclamation.

May 8, 1925.

The following Papers were read :—

1. "A Magnetic Bridge for Testing Straight Specimens and an Analysis of the Hysteresis Loop of Cobalt Chrome Steel," by **EDWARD HUGHES**, B.Sc.

2. "The Experimental Control of Electrically Broadened Spectral Lines," by M. C. **JOHNSON**, B.A.

3. "On the Spectra of the Metals of the Aluminium Sub-group," by M. K. **RAO**.

A **DEMONSTRATION** entitled "The Diffraction of Light by a Spherical Obstacle" was given by Prof. A. O. **RANKINE**, O.B.E., D.Sc.

May 22, 1925.

Prof. L. S. **ORNSTEIN**, of Utrecht, gave a lecture entitled "The Intensity of Spectral Lines—Measurement and Theory."

A vote of thanks to the Lecturer was proposed by Dr. **ALEXANDER RUSSELL**, F.R.S., and was carried by acclamation.

June 12, 1925.

The following Papers were read :—

1. "On Mass and Energy," by G. **TEMPLE**, Ph.D., Imperial College of Science."

2. "The Characteristic Curves of Liquid Jets," by E. **TYLER**, M.Sc., and E. G. **RICHARDSON**, M.Sc., University College, London.

The following **DEMONSTRATIONS** were given :—

"Some Experiments to Illustrate the Application of the Shakespeare Katharometer to Physical Research," by Dr. H. L. **DAYNES**, of the Cambridge Scientific Instrument Co.

"Some Simple Apparatus for the Estimation of Carbon Dioxide," by Dr. **EZER GRIFFITHS**, of the National Physical Laboratory.

June 26, 1925.

E. H. **Rayner**, M.A., Sc.D., in the Chair.

The following Papers were read :—

1. "The Electrostatic Capacity of Two Spheres when Touching One Another," by **Alexander Russell**, D.Sc., F.R.S., Principal of Faraday House.

2. "An Investigation of the Control Conditions under which Newton's Law is Valid for the Emission of Heat from Electrically Heated Wires," by Miss S. **Marshall** and J. O. C. **Vick**.

3. "Condensation Nuclei Produced by the Illumination of Air-Halogen Mixtures," by **Ivor Jones**, B.Sc., Garrod Thomas Research Student, University College. Aberystwyth.

July 4, 1925.

A SPECIAL MEETING was held at Oxford, by kind invitation of Prof. F. A. **Lindemann**, F.R.S., who entertained the Society at lunch in Christ Church Hall and at tea in Wadham College Hall. The meeting was held in the Clarendon Laboratory, the **President** being in the Chair.

The following Papers were read and illustrated by Demonstrations :—

1. "A Photoelectric Radiation Pyrometer," by Prof. F. A. **Lindemann**, F.R.S., and T. C. **Keeley**.

2. "The Determination of Ozone in the Upper Air," by G. M. B. **Dobson** and D. N. **Harrison**.

3. "The Absorption of Electrons in Metal Vapours," by R. B. **Brode**.

4. "Some Mercury Vapour Pumps," by M. W. **Garrett**.

5. "Two New Types of Low Pressure Gauges," by E. **Bolton King**.

6. "An Application of Spectrography to the Measurement of High Temperatures," by I. O. **Griffith**.

7. "X-Ray Analysis of Bent Rocksalt Crystals," by C. H. **Bosanquet**.

A cordial vote of thanks to Prof. Lindemann was proposed by the **President** and carried by acclamation.

I.—THE MAGNETISM OF ANNEALED CARBON STEELS.

By Prof. S. W. J. Smith, *F.R.S.*, A. A. Dee, *B.Sc.*, *D.I.C.*, and W. V. Mayneord, *M.Sc.*, University of Birmingham.

Read May 23, 1924.

Abstract.

This Paper is a sequel to one by S. W. J. Smith and J. Guild (Proc. Phys. Soc., 1912, Vol. 24, p. 342) on the "Self-Demagnetisation of Annealed Steel Rods," in which it was shown that the residual moment of a magnetised rod of annealed steel (of particular dimensions) does not diminish continuously towards zero as the temperature is raised, but changes sign at about 180°, reaches a maximum negative value at about 220°C., and subsequently slowly falls until the temperature at which steel ceases to be ferromagnetic is attained.

Experimental evidence is adduced to show that the reversal is primarily due to the laminated structure of the iron-iron carbide "eutectoid" which the steel contains.

It is shown that the reversal disappears when this structure is destroyed, and an explanation of the fact that the effect is most pronounced in a steel containing the eutectoid percentage of carbon is given.

Measurements have also been made to find how the intensity of the residual moment at 220°C., depends upon (1) the dimension-ratio of the bar, (2) the intensity of the field by which it is magnetised. The results are described and an interpretation is given.

It is suggested that measurements of the kind described could be employed to obtain rapidly and in a simple manner useful information concerning particular samples of steel.

I. Introductory.

In the years just preceding 1914, one of us, assisted by others, examined various properties of "steel" arising from the fact that it is a mixture of two or more substances each with its own magnetic characteristics.

It was pointed out in connexion with the results obtained that various problems of interest remained to be investigated. To these until recently it has not been possible to return. On the present occasion we desire to describe a few of the further experiments that have been made. These may be regarded as a continuation and elucidation of those described in the Paper on the "self-demagnetisation of annealed steel rods" read before the Physical Society in 1912.

Among other things, we hope to show that a criticism of these experiments which has been offered* rests upon a misunderstanding of their significance.

Although our interpretation of these particular experiments is practically the same now as it was then, it is possible that, in the desire to be brief, we did not express it as clearly as we might have done. We hope, however, that the new experiments about to be described, together with our explanation of them, will leave no room for doubt concerning the cause of the phenomena we described.

II. The Constitution of Annealed Carbon Steels.

Annealed carbon steels are magnetic mixtures of which the constitution and micro-structure are now well known.

Disregarding the "impurities," chiefly manganese and silicon, in those we have used, such steels contain only iron and its carbide Fe_3C. The amount of the latter contained in a particular steel is directly proportional to the percentage of carbon which it contains. If the steel contains *c* per cent. of carbon, the percentage of carbide present is approximately 15 *c*.

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* N. H. Williams, Phys. Rev., III, 115 (1914).

Instead of being uniformly distributed throughout the mixture, the carbide is intimately associated with a definite proportion of the iron. It is distributed with respect to this associated iron in alternating, roughly parallel layers. This complex of the two substances is the eutectoid mixture " pearlite " containing as a whole *e* per cent. of carbon, where *e* is approximately 0·9.

If the steel contains exactly the eutectoid percentage of carbon, it consists entirely of grains of the complex.

If *c* is less than *e*, i.e., if the steel is "hypo-eutectoid," it contains grains of the complex together with grains of " free " iron. The proportion in which these are present is such that the eutectoid grains constitute the fraction c/e of the whole mass.

If *c* is greater than *e*, i.e., if the steel is " hyper-eutectoid," it contains grains of the complex together with grains of " free " carbide, the fraction of the whole mass which consists of eutectoid grains being $(100-15c)/(100-15\,e)$.

The percentage of carbon contained in the steels examined by Mr. Guild and one of us ranged from about 0·15 in that containing least to about 1·5 in that containing most. In the former about one-sixth was eutectoid, the remainder being iron. In the latter about nine-tenths was eutectoid, the remainder being carbide. In the first, grains of pearlite occur in patches separated by grains of iron. The second consists of grains of pearlite together with grains of excess carbide, many of which occur as a network surrounding the pearlite grains.

III. Experiments Suggested by the Fact that Steel is a Magnetic Mixture.

The carbide, like iron, is ferro-magnetic ; * but, compared with the latter, it is magnetically hard. The field required to produce saturation in it (as it occurs in steel) is much greater than in iron and the retentivity is correspondingly greater.†

It seemed obvious that a careful study of the magnetic properties of a mixture of such substances would lead to interesting results.

One of the problems which suggested themselves was this : What will be the distribution of magnetism in a rod of such material, magnetised in the ordinary way ? How much will the iron and the carbide, respectively, contribute to the moment of the rod ?

It was thought that part at least of the answer to such questions might be found by making use of the fact that the carbide ceases to be ferro-magnetic‡ when heated above 215°C. It was proposed to measure the moment of the rod at room temperature and then to heat it continuously, observing how the moment varied. It seemed clear that some change in the slope of the magnetism-temperature curve would occur near 215°C. Below this temperature both constituents would be contributing to the observed moment ; above it the iron would be acting alone.

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* cf. Smith, Proc. Phys. Soc., 25, 77 (1912).

† cf. Smith and Guild, Proc. Phys. Soc., 24, 345 (1912).

‡ Smith, White and Barker, Proc. Phys. Soc., 24, 64 (1911) ; and Smith, Proc. Phys. Soc., 25, 77 (1912).

The results of the preliminary experiments arising out of these considerations are shown in Fig. 1 of the Paper by Smith and Guild (l.c., p. 344). Six rods containing different percentages of carbon, ranging from 0·15 to 1·53, were examined. They were all of the same dimensions, namely, 7 cm. long and 0·55 cm. in diameter. Each had an axial hole, of diameter 0·3 cm., drilled through it from end to end for convenience of temperature measurement. The impurities, chiefly manganese and silicon, were present to about the same amount in all.

In each case the moment fell at first, as expected, when the temperature rose; but, except in the steel containing the smallest percentage of carbon, the variation of the moment at the higher temperatures was quite different from what seemed likely to happen before the experiments were made. In every other steel the moment fell continuously until, in the neighbourhood of 180°C., it became zero. It then changed sign, ultimately reaching a maximum negative value near 220°C. From this it rose again towards zero, reaching that value when the temperature* at which steel loses its magnetism was attained.

IV. THE RESIDUAL MAGNETISM OF ANNEALED STEEL RODS.

The fact that the negative magnetism reached its maximum value at the point where the magnetism of the carbide disappeared made it obvious that it was carried by the iron. It remained to discover how this negative magnetism was acquired.

It was clearly not due to a reversal of the type discovered by Waltenhofen, for it was obtained when the conditions which this reversal requires were absent. This was stated quite explicitly in the Paper in which the experiments were described; but, according to Williams, the reversal only occurred in the rod he used when the Waltenhofen conditions were present, i.e., when the current used to magnetise his rod oscillated at break.

The rod he used had a very different dimension-ratio from ours and, although it contained 0·85 per cent. of carbon, nothing is said as to the conditions under which it was annealed. Williams attributes the reversal of moment beyond 180°C. to what happens in the outer layers of the rod and suggests that these will keep their magnetism longer than the interior when the temperature is raised because it is "almost inevitable that steel with such a high percentage of carbon should be slightly harder in its outside layers than at the interior of the specimen." Continuing, he says: "We should then expect that with rise of temperature the magnetism of the outer layers would persist after that of the softer interior had disappeared and hence that the resultant magnetic moment would be reversed at the high temperature."

Disregarding the fact that the reversal was obtained in steels containing percentages of carbon much lower than 0·85, it may be pointed out that, unless precautions are taken to avoid decarburisation during annealing, the outer layers may easily be magnetically softer than the interior. Apart from this, the proposed interpretation leaves unexplained the fact that the reversal occurs at a temperature evidently closely related to the transition temperature of the carbide.

Soon after the results obtained by Williams were published, one of us had a rod constructed, from steel of unknown composition, of which the dimensions were nearly the same as those of the rod he used. This rod, which was 7 cm. long, had a cylinder hole just exceeding 0·55 cm. in diameter drilled through it axially. Into this could be fitted closely a rod of the same steel whose diameter was 0·55 cm. and length 7 cm. This composite rod showed no reversal of magnetism when heated, whether magnetised by the method originally used or by slowly withdrawing it from a solenoid according to the method adopted by Williams. Neither did the larger part of the composite rod when separately magnetised. The composite rod was next remagnetised as before and the inner rod was now withdrawn. On being tested it gave a reversal of moment of the kind originally described. It is obvious that neither outer skin nor eddy currents could be the cause of this result.

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* Smith and Guild, Phil. Trans. A, 220, 177 (1914).

V. THE CAUSE OF THE CHANGE IN SIGN OF THE RESIDUAL MOMENT.

We shall now attempt to show, more explicitly than in the earlier Paper, how, in our opinion, the reversal is produced.

The primary cause of the effect is the lamination of the eutectoid, i.e., the existence within the steel of alternating thin plates of two different substances, one magnetically hard and the other magnetically soft.

The fact that the negative magnetism is most pronounced in the steel containing nearly 0·9 per cent. of carbon is enough to suggest this. It is the only steel which is composed entirely of laminæ. But, as we shall see, much more direct proofs than this can be found. Before turning to these, a few remarks may be made concerning the effects which lamination might be expected to cause.

To take the simplest case, imagine two rods of the same size, one magnetically hard and the other magnetically soft, placed side by side to form a composite bar. Let this bar be magnetised by placing it in a field parallel to its length. Upon withdrawal of the field, each half will be subject to the demagnetising influence of its own residual magnetism. The extent to which this is effective will depend upon the ratio of the length of each rod to its breadth. The shorter the rod in comparison with its breadth the more effective will the demagnetising field be; but no rod, however short, can reduce its own moment to zero. Still less can it reverse the sign of that moment. But, besides acting upon itself, each rod acts upon the other. It is the rod of the relatively soft material that fares worst in this encounter. Hence it is that the polarity of the softer material may be reversed while that of the harder retains its original sign.

The conditions under which reversal of this type occurs can easily be studied by means of a model consisting of rods of iron and steel respectively. Experiments of this kind made for a particular purpose are described later in § X.

It can be seen that a composite bar consisting of alternate laminæ of hard and soft materials will possess magnetic properties similar in character to those of the simple model described above. Models to represent such cases are not difficult to construct and to use. For example, to show, as could be predicted, that the reversal produced by a given field in a rod of given size is more pronounced when the laminations are fine than when they are coarse.

The magnetic conditions within a rod of steel can never be as simple as in models of the kind just described since it probably never happens that the orientation of the iron and the carbide is the same in all the grains of eutectoid which the steel contains. In a fraction only of these will the laminæ lie in planes containing the direction of the magnetising field. In all others, the effects produced will be indirect, the extreme case being that of the grains whose laminations are perpendicular to the direction of the field.

We can, however, form a rough picture of the magnetic structure of any rod by regarding it as equivalent to a series of undulating laminated surfaces of which some are interrupted at different places in their course from one end of the rod to the other.

It would not be impossible to construct and examine a model with a structure of this character ; but for our present purpose it is unnecessary to do so, because it is easy by thermal treatment to convert a steel in which the lamination is complete into one in which there is none. If in the latter case reversal does not occur, we obtain a direct proof that lamination is its cause.

VI. EXPERIMENTAL PROOF THAT REVERSAL DOES NOT OCCUR WHEN LAMINATIONS ARE ABSENT.

We can begin by taking, for example, a eutectoid steel in which, by moderately slow cooling from about 750°C., the laminated structure, produced at about 700°C., is fully developed and observing the subsequent variation of its residual magnetism with temperature rise. The same specimen can now be heated to about 750°C. and cooled suddenly by quenching in water. By this means the lamination is suppressed. After this treatment the specimen is heated to a temperature not exceeding 700°C., maintained at this temperature for a few hours and then allowed to cool to room temperature. It is known from microscopic examination that the steel now consists of granules of carbide in a matrix of iron. If our interpretation of the phenomena be correct, the steel in this condition should show no reversal of residual magnetism when, after magnetisation at room temperature, it is heated to 200°C.

Experiments of this kind were performed upon a steel containing about 0·8 per cent. of carbon, with the results shown in Fig. 1.

The rod was of the same dimensions as those used in the earlier experiments. The abscissæ represent temperatures and the ordinates residual magnetism (in arbitrary units) as before.

The lowest curve shows the behaviour of the steel after slow cooling from 780°C. Direct microscopic examination, after the usual polishing and etching, showed that the steel consisted almost entirely of lamellar pearlite. The reversal of sign of the residual moment occurs exactly as in the earlier experiments.

The other curves represent the data obtained when, after quenching, the same rod had been heated for some time to temperatures of 400°C., 500°C., 600°C. and 680°C., respectively, having been allowed to cool to room temperature between successive heatings.

In none of them, as will be seen, does reversal occur.

After the experiments the specimen was again examined microscopically and seen to contain no laminations. It possessed the structure already described as characteristic of steel which has undergone thermal treatment of the kind to which it was subjected.

It is, of course, easy to see why reversal should not occur in steel of this structure. Its properties are now, in fact, very like those one of us half expected to find, as stated in § III., before any experiments upon this subject were made.

VII. PRACTICAL APPLICATIONS.

When the results shown in Fig. 1 are regarded from the practical standpoint, it will be seen how easy it is to determine by this magnetic method of examination whether a particular steel is in the laminated or in the granular condition. It is of interest in this connexion to note that the character of the laminæ in a particular steel can be altered considerably by thermal treatment less drastic than quenching, with subsequent tempering.

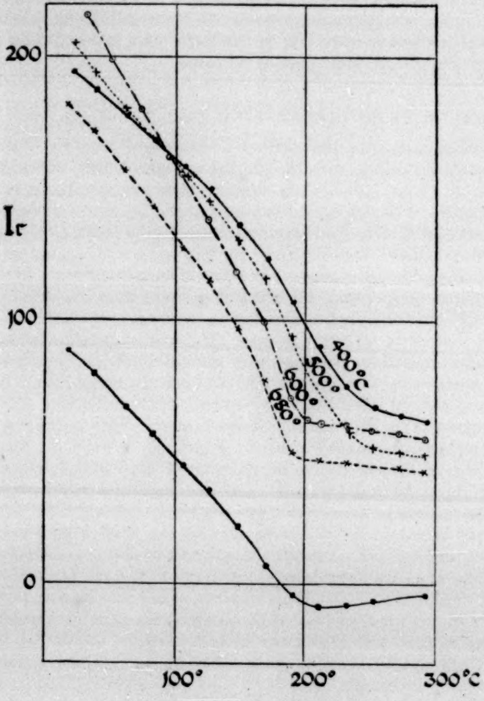

FIG. 1.

For instance, when steel after heating to 780°C. is taken out of the furnace and cooled in air instead of in the furnace, it is found by microscopic examination that the laminæ are of finer texture than those which result from the slower rate of cooling. On the other hand, if the steel during cooling from 780°C. is kept for a long time at a temperature just below that at which the laminations are produced, it is found that these laminations gradually break up under the influence of surface forces which give rise to structural instability. In consequence, steel treated in this way is partly laminar and partly granular when allowed to cool, and therefore intermediate in structure between an ordinary annealed steel and one tempered after quenching.

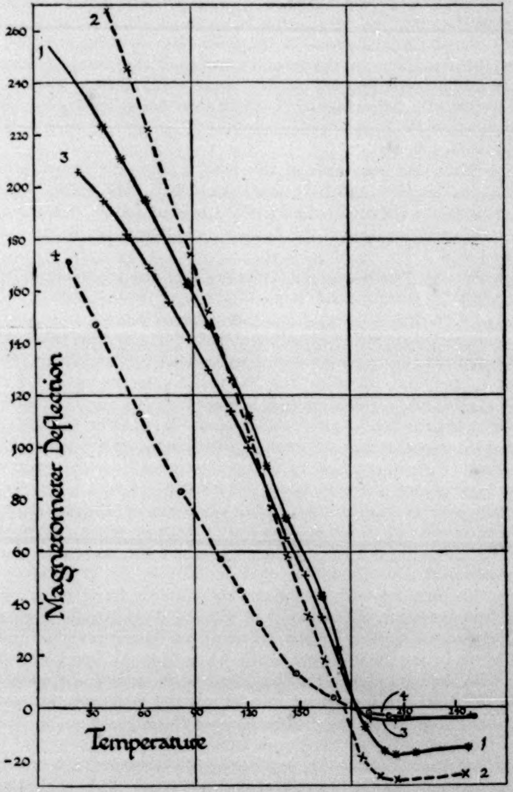

FIG. 2.

Experiments were made to find how such structural differences influence the temperature variation of residual magnetism.

Some of the results are shown in Fig. 2, which relates to a cylindrical rod of steel containing 0·9 per cent. of carbon, together with 0·29 per cent. of manganese, and 0·27 per cent. of silicon. Its length was 7 cm., and its radius 0·72 cm. A hole of 3 mm. diameter, to take the thermo-couple, was drilled through it axially.

It was magnetised in a solenoidal field of about 800 c.g.s. units, from which it was withdrawn in each experiment while the current was still flowing. The ordinates represent the residual moment of the rod in arbitrary units, the same in each experiment.

Curve 1 shows the behaviour of the steel after heating to 780°C., keeping at this temperature for about twenty minutes and then allowing to cool in the furnace, about thirty minutes elapsing before the temperature reached 400°C.

Curve 2 shows the behaviour of the same steel after heating to 780°C., keeping at this temperature for about twenty minutes as before and then taking out of the furnace and cooling in air.

Curve 3 shows the behaviour of the steel when, after heating to 780°C., the temperature was lowered and held between 650°C. and 670°C. for eight hours. The great reduction in the reversed magnetic moment (above 180°C.), corresponding with the partial disintegration of the laminæ, is apparent when Curve 3 is compared with Curves 1 and 2.

It is known that " cold working " of steel containing laminated pearlite, before heating to 680°C., greatly accelerates the rate of disintegration of the pearlite. Corresponding with this, it is found that the reversal disappears in such steels when, after cold-working, they are re-heated and kept for a comparatively short time at 680°C. We omit further reference to experiments of this kind for the present. It is sufficient to say that the effects of thermal treatment upon internal structure can be seen at once when the magnetism is measured.

Curve 4 shows the behaviour of the steel of Fig. 2, after heating to 950°C. and keeping at this temperature for three hours with subsequent cooling in the furnace. This treatment is known to lead to a characteristically coarse structure in which " minimum interlocking of the iron and the carbide " occurs. It is seen in this case that the reversal of residual magnetism has again almost disappeared.

This Curve exhibits another interesting feature when compared with that obtained after annealing at 780°C. The angle which the tangent to the curve makes with the horizontal axis diminishes continuously as the temperature rises. The curve shares this property with that given by Williams, for a rod of " annealed steel containing 0·85 per cent. of carbon " of similar dimensions. This suggests that the annealing process applied by him, of which no details are given, was not of the kind used by us in the experiments which he criticises. Apart from this, it is important to note that even when the treatment we described is applied to a rod, like that of Fig. 2, of dimensions similar to that used by Williams, the reversal is much less pronounced than when the dimensions are those given in the Paper to which he refers.

It is curious that Williams did not attempt to reproduce our conditions exactly, when examining our results, instead of endeavouring to show that oscillations at break of the magnetising current would produce or accentuate the reversal. This point was never in question. What we stated was that reversal occurred, under specified conditions, when such oscillations were avoided in the way we described. It also occurs when the particular method of avoiding oscillations described by him is used.

VIII. THE DEPENDENCE OF THE REVERSED MOMENT UPON THE PERCENTAGE OF CARBON PRESENT.

Before discussing how the dimensions of the rod affect the results, we may indicate briefly how the extent of the reversal may be expected to depend upon the percentage of carbon which the steel contains.

The amount of " free " iron, not associated with the carbide as eutectoid, increases continuously as the percentage of carbon falls below 0·9. Since the carbide of the eutectoid cannot in general produce reversal in this iron, it can be seen that the negative moment at 215°C., representing that of the reversed iron, will become an increasingly smaller fraction of the original residual moment at 15°C. as the percentage of carbon present is reduced.

Similarly when the percentage of carbon increases above 0·9, increasing amounts of " free " carbide appear. Some at least of this will be disposed with respect to the eutectoid grains as an enveloping net work, i.e., in such a way as to lessen the reversing effect of their carbide laminæ upon the iron laminæ which they contain.

It would also appear that the laminæ of hyper-eutectoid steel are frequently coarser in texture than those of hypo-eutectoid steel. This might also diminish the effectiveness of the carbide laminæ in producing reversal of the associated iron.

Further investigations of such points might be made experimentally, but from the experiments already described and the general considerations just given, it is probable that in a series of steels, similar in character and in previous thermal treatment, the maximum reversal will occur in that containing the eutectoid percentage of carbon.

It is, of course, easy to see why, in a completely granulated steel, reversal will in general be absent. Sometimes, however, there may be a regular linear distribution of granules which can produce reversal. It may also happen, in hyper-eutectoid steel, that, although pre-existing pearlite has been granulated, the excess-carbide has escaped complete granulation. In such steel, it is possible that reversal may occur. We have made a beginning upon experiments concerned with such questions ; but need not refer to them further at present.

IX. THE DEPENDENCE OF THE REVERSED MOMENT UPON THE DIMENSION-RATIO OF THE ROD.

We turn now to the consideration of the question : How should the intensity of the residual negative polarity shown at 215°C. depend, in a given steel, upon the dimension-ratio of the rod employed ?

We can see, in a general way, what answer to expect by considering again the simple model of hard and soft rods placed side by side. When the length of the composite rod is great compared with its diameter, i.e., when the dimension-ratio l/d is large, the demagnetising effect of each half upon itself as well as upon the other will be relatively small. Hence we should not expect reversal in either.

Conversely, when the ratio l/d is very small, the demagnetising effect of each half upon itself will be so great that even in the harder half the resultant magnetic intensity will be only a small fraction of that existing before the magnetising field is removed. Consequently the negative intensity which this half can induce in the other must also be small, since the polarity of the rod as a whole cannot change sign. What happens in these and in intermediate cases can be followed in greater detail by considering the I H curve for each material in conjunction with the demagnetisation coefficients, experimentally known, for each dimension-ratio.

Applying such considerations to steel, regarded as a laminated substance, it can be seen that the residual negative intensity at 215°C. will pass through a maximum value as l/d is decreased continuously, i.e., there will be a particular dimension-ratio for which the phenomenon of reversal is most pronounced.

In Fig. 3 are collected the results of experiments made to test this inference.

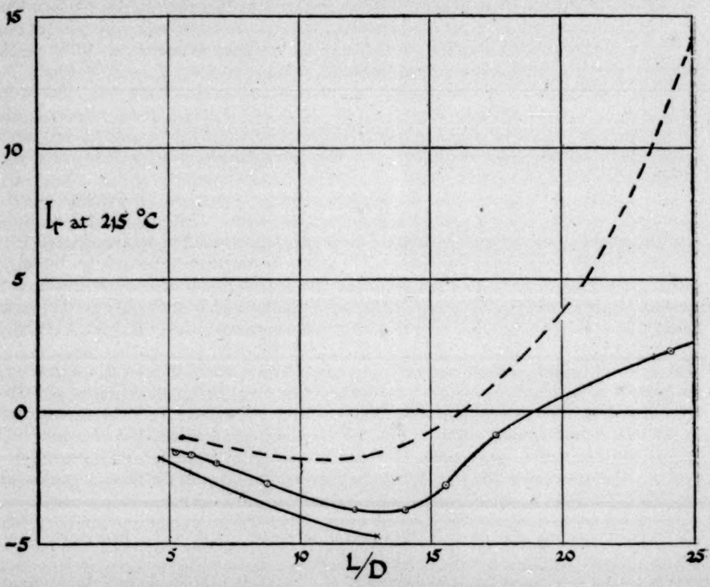

FIG. 3.

The dotted curve represents a few preliminary results obtained by using a rod of tool-steel containing about 1 per cent. of carbon, about half a centimetre in diameter and originally about 20 cm. long. At its original length, the residual intensity at 215°C. was positive and about thirty times as great as the maximum negative intensity shown in the Figure. This residual intensity fell rapidly as the rod was shortened ; but the measurements made were so widely spaced that they serve only to indicate generally what happens. It is seen, however, that somewhere between $l/d=20$ and $l/d=15$, the residual magnetism changes sign and that the negative value is less when $l/d=5$ than when it is 13.

In this case the values of the residual magnetism are given in arbitrary units chosen for convenience of representation on the same diagram as the other curves. In these, the intensity of magnetisation is expressed in c.g.s. units.

The lowest curve represents experiments made upon a rod of which the length was kept constant at 7 cm., while the dimension-ratio was increased from one measurement to the next by reducing the diameter on the lathe.

This steel had the same composition as that used in the experiments of Fig. 2 and was in the condition following slow cooling from 780°C. as in Curve 1 of that Figure. The magnetisation at room temperature was carried out in the same way as in the experiments of Fig. 1. The initial diameter of the rod was 1·5 cm. The final diameter was 0·54 cm. The initial and final dimension-ratios were thus approximately 4·7 and 13.

It will be seen that the negative intensity increases at a gradually decreasing rate as the dimension-ratio is increased. The original ratio approximates to that used by Williams, while the final one approximates to that used by Smith and Guild. The rod had the usual axial hole of 0·3 cm. diameter.

The middle curve shows a more complete series of observations obtained in another way.

In this case the experiments were made upon a rod of adjustable diameter consisting of a solid core and a series of concentric tubes fitting closely upon one another, made from the same steel. This contained 0·77 per cent. of carbon, the core and tubes being annealed beforehand by cooling in the furnace from 780°C. The length and diameter of the core were 7 cm. and 0·29 cm. respectively. The tubes were of the same length as the core, and when all were in position, the arrangement was equivalent to a rod 7 cm. long and 1·34 cm. in diameter.

It is not to be expected that this method of procedure will give exactly the same results as would be got from a rod of the same material turned down in the lathe between successive measurements of the residual intensity. It is, however, of interest from the fact that the conditions of annealing are different and can be altered at will, and that observations with a given dimension-ratio can be repeated at any time during the course of the experiments. In the particular set of measurements represented, the existence of a maximum negative intensity is clearly shown. It occurs when the dimension-ratio is about 13. The negative polarity disappears when l/d exceeds 19.

X. THE DEPENDENCE OF THE REVERSED MOMENT IN A LAMINATED ROD UPON THE MAGNETISING FIELD.

One other question remains: How, for a given rod of steel, does the sign and intensity of the residual magnetism at 215°C. depend upon the strength of the field used to magnetise it at 15°C.?

The answer can, of course, be found by direct experiment; but before giving the results obtained it is convenient to examine the behaviour of a model of the kind already described, consisting of rods of hard and soft materials placed side by side. The results obtained using steel can afterwards be compared with those obtained in this simplest possible case of lamination. The extent to which the views expressed in preceding sections are confirmed by experiments of this type will then be clear.

The results shown in Fig. 4 were obtained by the use of a composite cylindrical rod, consisting of half-cylinders of iron and steel respectively, each 7 cm. long and of 0·3 cm. radius, in contact with each other along their plane rectangular surfaces.

For each experiment this composite rod, after demagnetisation of each half, was magnetised by means of a solenoid through which a measured current was passing. After withdrawal from the solenoidal field, the residual moment of the bar was measured in arbitrary units. The brass clip which held the two halves together was now removed and they were carefully separated, being kept parallel to one another while their centres moved apart along a line perpendicular to the direction of magnetisation. The residual moment of each half was now measured in the same way as that of the whole rod.

The results, for different values of the solenoidal field in c.g.s. units are shown in the Figure.

It would take too long to deal with the significance of every detail of these results. We shall confine ourselves to their general interpretation, which is all that is required for present purposes.

In the weakest fields as will be seen, the residual moment of the iron half is practically the same as that of the whole rod, while that of the steel half is relatively very small. On account of its much greater magnetic hardness, the response of the steel to such magnetising fields is very small compared with that of the iron. When the field is withdrawn, the steel's own demagnetising field aided by that of the iron reduces its moment practically to zero. The demagnetising field on the iron is thus practically the same in the composite bar as it is when the steel is removed. Consequently its moment will remain practically unchanged upon that removal and will be same as that previously shown by the whole rod. That of the steel, however, will rise a little in consequence of the removal of the reversing field previously exerted by the iron. Accordingly, as will be seen, the sum of the moments shown

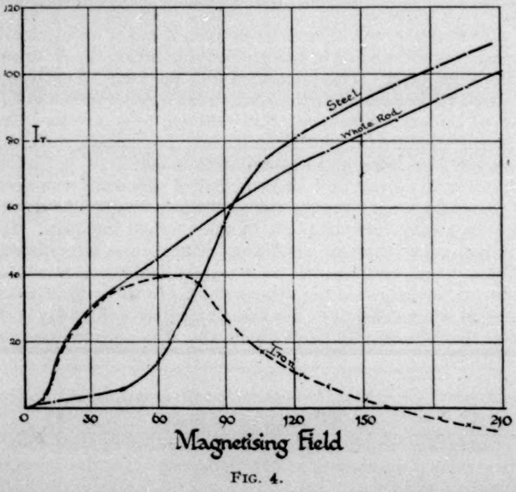

FIG. 4.

by the two halves will be, from the outset, greater than that shown by the rod as a whole.

Although the residual moment of the softer half rises rapidly, at first, as the magnetising field is increased, the rate of rise soon slackens and simultaneously that of the steel half begins to increase. The "second stage" in the magnetisation curve for the iron is nearing completion when that for the steel begins.

In a field slightly less than 80 c.g.s. units, the residual moment of the steel has become equal to that of the iron, the sum of the two being now much greater than that of the rod as a whole. The moment of each half has evidently risen considerably when the demagnetising field due to the other has been removed by separating them.

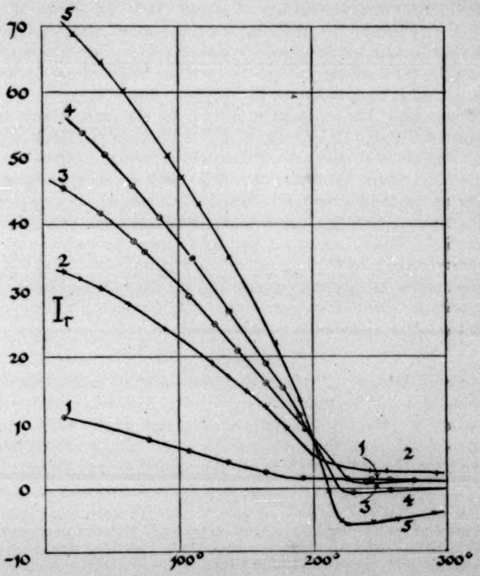

FIG. 5.

From this point the residual moment of the iron falls rapidly, while that of the steel continues to rise. After magnetisation in a field of about 90 units, the residual moment of the steel is as great as that of the whole rod, while that of the iron is less than half this amount, being at the same time, no doubt, considerably greater than it was before separation from the steel. After magnetisation in a field of about 160 units, the residual moment of the iron is zero, although, no doubt, it was negative before the demagnetising field due to the steel was removed; just as the positive moment of the steel is presumably less positive now than it was before the iron was taken away.

Finally, in still larger fields, the residual moment of the iron is negative. The reversal produced by the steel is now so great that it persists when the latter is removed.

XI. EXPERIMENTAL RESULTS FOR ANNEALED EUTECTOID STEEL.

The experiments, of which the results are collected in Fig. 5, were made upon a rod 7 cm. long and 0·55 cm. diameter, containing 0·85 per cent. of carbon. The ordinates represent residual moments in arbitrary units, the abscissæ being temperatures in degrees Centigrade.

The curves show how the residual moment varied while the temperature was raised to 300°C., after magnetisation at about 15°C. in fields of different intensities. Curve 1 represents the behaviour of the steel after magnetisation in the weakest field that could be employed conveniently. Curve 5 was obtained after magnetisation in a field strong enough to produce approximate saturation. Intermediate fields gave the results shown in Curves 2, 3 and 4.

It will be seen that the magnetism shown by the iron, above the temperature at which the carbide ceases to be ferro-magnetic, is positive when weak magnetising fields are used and rises at first when the field intensity is increased (cf. 1 and 2). Subsequently it falls again (cf. 2 and 3); ultimately becoming negative to an extent which increases as the field strength is raised (cf. 4 and 5).

Except for the fact that the iron which it contains is heated, raising the temperature of the steel to 220°C. separates the iron from the carbide in practically the same way as it was separated from the steel in the composite rod of § X. Accordingly, if annealed steel owes its magnetic properties to alternating laminæ of iron and its carbide, relatively soft and hard at 15°C., the interpretation of its behaviour in the experiments of Fig. 5 presents no difficulty.

XII.

Further consequences of the existence of this condition in steel have suggested themselves and have been examined. Of these perhaps the most interesting occur when the steel is magnetised at temperatures above the critical temperature of the carbide and then allowed to cool. These experiments were begun by one of us and Mr. Guild.* We hope to describe them upon some future occasion.

DISCUSSION.

Mr. J. GUILD said that it came as a pleasant surprise to him to find that the Paper dealt with an extension of the work carried out some years ago by Prof. Smith and himself—and he was very glad that their original explanation of the facts had been vindicated so clearly. This explanation when first put forward had been rather contumeliously criticised, particularly by Williams, who proposed an alternative theory. A number of phenomena could be quoted in support of the original view, but instead of detailing these he would refer to some further work which had not been published. Williams had tried to repeat some of the experiments with a large thick rod, and had found that the polarity-reversal effect occurred if the magnetising field was broken suddenly but not if the specimen was gradually withdrawn from the field. He based a theory on this fact and wished to explain the original experiments by means of it, but as a matter of fact the conditions assumed by him did not obtain in the experiments in question. The speaker tried to repeat Williams' results with rods of various sizes, and found that their character was reversed when small rods were used. He had been able to trace the effect of the dimensions of the specimen on the results obtained, but had been obliged by the pressure of other work to abandon the investigation. The fresh light on the matter contained in the Paper now before the meeting should make it possible to explain this effect.

The PRESIDENT thanked the authors for an interesting and conclusive Paper. He expressed the great pleasure felt by the Fellows present at having Prof. Smith once more amongst them.

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* Smith and Guild, l.c., pp. 346, 7.

II.—UNDERBLOWN PIPES.

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Received June 3, 1924.

(*Communicated by* LORD RAYLEIGH.)

ABSTRACT.

The phenomena of underblown pipes has been studied in the case of four wooden pipes ranging over an octave. It has been found that it presents many features of remarkable interest. The existing theories regarding the phenomena have been discussed, and a new theory is proposed which attempts at once to explain both these phenomena.

INTRODUCTION.

THE behaviour of flute organ pipes with different blowing pressures presents peculiarities which have so far been mathematically unaccounted for. It is a matter of common knowledge that an organ pipe gives out its fundamental note with a particular blowing pressure. This pressure to elicit the fundamental is, however, different for different pipes, and even with the same pipe it largely depends on the details of its make and the distance of the slit from the lip. Though the phenomena of overblown pipes have been known for a long time, no definite advances in theory appear to have been successful in explaining them. In recent years some consideration has been given to this subject by Arthur C. Lunn,* V. Lough,† and Bhargava and Ghosh.‡

A. C. Lunn indicated in his Paper a short theory of the phenomena of overblowing, which for want of necessary experimental data could not be verified satisfactorily. The rough values which he himself has given are found to be at variance with the theory put forward. The present Paper is the result of an attempt on the part of the authors to obtain sufficient data whereby to test the validity of his theory. In the course of their experiments they have found that, even before the pressure rises sufficiently to elicit the fundamental, the organ pipe gives out notes, no less interesting than those of the overblown. Hence it is proposed to examine these phenomena of "underblowing" in greater detail and to put forward a theory which shall embrace the phenomena of overblowing also. The last-mentioned effect was for the first time recognised by the late Lord Rayleigh,§ who with a metal open pipe of frequency 255, and using the method of Blaikley,‖ arrived at the variation of pitch under a gradually decreasing blowing pressure.

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* Phys. Rev., May (1920).

† Phil. Mag., Jan. (1922).

‡ Phys. Rev., Nov. (1922).

§ Phil. Mag., 13 (1882).

‖ Phil. Mag. (1879).

EXPERIMENTAL.

The experiments were conducted with four wooden open pipes of frequencies 256, 320, 384 and 512. The pressures, obtained by an acoustic bellows with a large wind-chest, were gradually increased, and could be maintained constant at any value. At any instant the pressure was measured by a water manometer, using a cathetometer microscope. The pitch of the note at any stage was found by tuning a monochord to unison by an expert musical ear. The values are tabulated below :—

TABLE OF VALUES.—*Pressures are in cm. of water.*

Pitch of the pipe.	Underblown tone corresponding to fundamental.				Underblown tone corresponding to octave.				Natural fundamental commences at		Pressure for frequency equal to the actual face value.
	Commences at		Ceases at		Commences at		Ceases at				
	Pressure	Frequency	Pressure	Frequency	Pressure	Frequency	Pressure	Frequency	Pressure	Frequency	
256	0·10	247·9	0·65	271·0	0·65	529·3	1·4	550·7	1·4	245·8	2·0
320	0·20	283·0	0·80	341·7	0·80	631·9	2·4	...	2·4	297·3	3·2
384	0·30	281·4	1·20	402·2	*Elicited with difficulty.*		*Uncertain.*		3·2	312·4	4·2
512	0·90	507·0	2·2	539·5	*No octave counterpart.*				6·2	482·7	9·2

CONCLUSIONS.

From the above results we are led to conclude that the behaviour described by Lord Rayleigh is characteristic of pipes with frequencies lower than a limiting frequency. The behaviour of the 256 pipe is similar to that described by him, that of 320 shows a slight difference, while in the other two the difference becomes marked at least in one respect. In all the pipes a note distinctly lower in pitch as well as in intensity than the fundamental makes its appearance at a few millimetres pressure. The minimum pressure to start this note is higher the higher the pitch. This note gets considerably sharper than the fundamental as the pressure rises, and ceases at pressures which are also higher the higher the frequencies. Then in the two lower pipes a note slightly lower than the octave starts a little before or just as the fundamental ceases. In the highest of our set, instead of the octave counterpart a period of silence ensues. This octave counterpart is very feeble in the 320 pipe, and is elicited with difficulty. In the 384 it is produced with considerable difficulty, and ceases abruptly, this being followed by a period of silence. The periods of silence in the two higher pipes are followed by the normal tones, which are, to start with, a bit flatter than the fundamentals. The pressures at which the normal tones commence are also higher for higher pipes. In the lower pipes the octave tones which started as the underblown fundamentals disappear, rise with pressure and cease, giving rise to the normal notes.

THEORY AND DISCUSSION.

We shall, in the first place, consider the instability of the fluid jet, as this forms the main characteristic on which Helmholtz based his explanation of the initiation and maintenance of vibrations in organ pipes. Lord Rayleigh* obtained the conditions relating to pressure at the surface of separation of two fluid masses moving with different velocities, the disturbance being supposed to involve x and t through the factors e^{ikx} and e^{int} as

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* Theory of Sound, Vol. 2, pp. 377-78.

$$g\,(\rho-\rho^1)+k^2T = k\rho\,(v+n/k)^2 \coth kl \\ +k\rho^1\,(v^1+n/k)^2 \coth kl'.$$

He obtained for the criterion of stability

$$(\rho \coth kl+\rho' \coth kl')\ \{g\,(\rho-\rho')+Tk^2\}-k\rho\rho' \coth kl\,.\coth kl'\,.(v-v')^2 > 0$$

From this it follows that when g and T both vanish the motion is unstable for all disturbances, that is, for all values of k. This, with the necessary substitutions, can be easily adapted to the present case of a plane jet of fluid playing into a stationary fluid of its own density.

Both Helmholtz and Lord Rayleigh agree in the statement that the surfaces of the thin blade of air are vortex sheets, and Prof. Tyndall's experiments on burning gas jets establish the fact beyond doubt. The instability of the blade of air might indeed be attributable to the existence of vortices as Helmholtz and Lord Rayleigh had opined, and might, in consequence, easily be conceived to increase with pressure. The vortices further serve as an accelerating force, with periodically alternating direction on the mass of air in the pipe, with the result that the period of the pipe is brought down when their strength increases on stronger blowing. An explanation of the phenomena of overblown pipes may, in fact, be suggested on these lines. The period becomes smaller and smaller for pressures higher than the fundamental pressure until at a particular stage it settles down rather suddenly to that of the octave, the next higher mode of free vibration of the pipe. If, as Lough on the one hand and Bhargava and Ghosh on the other have explained, these vortex pulsations are to be endowed with a free period of their own, this period must perhaps be assumed to undergo transformations similar to the changes of the tone of the pipe as a whole.

Next we have to take into account that the blade, though very thin, has finite thickness, and takes time to travel past the lip as well as the width of the mouth of the pipe. We may suppose that at the fundamental pressure the corresponding width and velocity of the stream are such as to make the time of transit to and fro exactly equal to the time which a compression or rarefaction takes to travel twice the length of the pipe. For higher pressures we know that the velocity and the width of the stream increase, but the former more rapidly than the latter. Now the stream produces a compression or rarefaction a little before the proper phase is reached, and thus reduces the wave-length. This continues until the pipe prefers to jump into the higher mode of free vibration. The variations of the velocity and the width of the stream, each in their own way, render it possible for them to have at other pressures values which make the time of transit of the blade to and fro again equal to the periodic time of the pipe. This happens at the underblown fundamental pressure. Under these circumstances we expect to find here also a counterpart of the phenomena observed in the neighbourhood of the normal fundamental, and this is amply borne out by experiment. The energy supplied being very small, the tones here are necessarily feeble.

In conclusion the authors wish to express their best thanks to Prof. A. L Narayan for his interest and suggestions during the progress of the work, and also to Prof. D. Venkataswamy Naidu, of the Maharajah's Music College, for his assistance in the experimental portion of the work.

DEMONSTRATION.

In the course of his demonstration Mr. **Blaikley** stated that the upper partials actually given by an organ pipe are not strictly in tune with the notes which would be obtained from the harmonic chord on the fundamental. The pitch of the actual notes is sharper than the pitch calculated from the length of the pipe, owing partly to the correction for the open end, but still more to the correction for imperfect closure at the lip end, the latter correction being different for different partials. By resonance to two tuning forks Mr. Blaikley showed that a pipe having a prime of 256 vibrations by resonance gave a second partial of 530 instead of 512, i.e., a note more than a quarter of a tone sharper than the octave of the prime. The dimensions of this pipe were : Length from upper lip to end, 55·4 cm. ; diameter, 5·28 cm. ; mouth : width, 3·5 cm., height, 0·88 cm. ; slit or flue, 0·15 cm. The blade of air impinging on the lip acts as a reed, and the pitch of the note actually obtained is determined by the interaction between the blade or reed and the pipe ; it cannot be calculated from the natural frequency of one of these elements without reference to the other.

The pipe was next sounded by blowing it steadily with a pump at gradually increasing pressure, the latter being measured with a water manometer. Steady sound was first obtained at 0·28 cm., the pitch being 250, and therefore flatter than the natural fundamental as determined by maximum resonance. At 0·38 cm. the pitch was 259, and the simple tone ceased. At 0·45 cm. the octave sounded with the prime pitch 271, and between 0·45 cm. and 0·81 cm. there was silence. At 0·81 cm. the underblown octave appeared, pitch 516, and at about 1 cm. a critical condition appeared, a mere waft of air over the lip changing the pitch from prime to octave. Between 1 cm. and 1·82 cm. there was silence, and at 2·35 the compound tone 256 sounded, this note gradually rising with increasing pressure. The resonant or natural pitch of the prime being 256 vibrations and the pitch by blowing rising from 250 to 259 with increase of pressure from 0·28 cm. to 0·38 cm., it follows that the true pitch of the pure fundamental is obtained from an intermediate pressure, between 0·28 cm. and 0·38 cm.

With the comparatively high or customary pressures the tone is compound, and the pitch is determined not only by the pressure, but by the presence of the higher proper tones, which are sharper than the harmonic partials of the true resonant prime. The resulting compound tone is therefore in some degree a forced vibration.

In a further experiment two harmonium reeds of about 1,024 vibs. were used, being blown steadily. To one of these a very short length of tube with an adjustable slider was attached, by means of which the varying influence of the tube upon the pitch of the reed was shown. Further, at a certain length of tube sound ceased, but the addition of a quarter, or any odd number of quarter wave-lengths of tube restored speech, whereas with added half wave-lengths silence continued.

DISCUSSION.

Mr. E. G. **Richardson** referred to the authors' attempt to connect the pressure of the air supply with the width of the air blade and the frequency of the vorticity. The blade is assumed to set up vortices, successive vortices being on opposite sides of it, so that the " frequency of the vorticity " is equal to the quotient of the velocity of the vortices relative to the lip by the distance between successive vortices ; this frequency will, therefore, depend on the velocity of the air blade. A relation of the kind referred to has been obtained by Kruger and his assistants at Greifswald (Annalen der Physik, vol. 60, 1919), who connected the frequency of the note obtained with the velocity of efflux of the air stream and the distance from lip to languid. Earlier experiments on this subject were made by Wachsmüth and Von Schaik in 1905. The speaker had been making similar investigations with a pipe having an annular slit similar to that of the Galton whistle, and had obtained similar results. He suggested that an organ-pipe, when underblown, was responding to a frequency of production of vortices which was a sub-multiple of that of the fundamental of the pipe. He congratulated the authors on their work, and hoped they would be able to put into a more quantitative form the theory outlined at the end of their Paper.

Mr. W. A. **Benton** said that the lowest pressure ever actually used in organs was one of 1½ in. found in an early Italian chamber organ ; below such a pressure it would be difficult to get a steady tone. The maximum pressure used in practice is about 3½ in. He suggested that the authors should study a set of pipes all designed to give their fundamentals at the same pressure, e.g., a set forming an organ stop ; it would be well if the authors were to mention the height of the mouth and the width of the upper lip in the pipes used by them. The vibration of the metal lip is of small consequence ; it is the air blade that has to be considered.

Mr. W. E. **Benton** (communicated): The work described would be of use to other investigators if full particulars of the pipes under examination were given. Of these, the height of the mouth, the depth and breadth of the pipe, the exact position of the upper lip with respect to the wind-sheet, the breadth and form of the slit, the dimensions of the throat and the diameter of the foot are all necessary for any proper interpretation of the results. For instance, if measurements of the mean velocity of the jet for any given pressure had been made with a hot-wire anemometer, it could have been shown that the note given by the pipe is approximately equal to $\frac{iv}{2{\cdot}3a}$ where i is an integer, v is the mean velocity of the jet, and a is the height of the mouth, provided that the breadth of the slit were not more than about 1 mm.

Without such measurements the observations cannot serve to support or contradict any theory whatever.

Lord Rayleigh's criterion of stability is inapplicable here, since V. Hensen's Pitot-tube measurements of the distribution of velocity in a laminar jet proved twenty years ago that there is no surface of discontinuity between a laminar gaseous jet and the surrounding air. That his observations give nearly correct values of the velocity can be shown by integrating for the total momentum across any section, perpendicular to the principal plane of the jet. The work of A. C. Lunn is not applicable to an organ pipe, since the "Schlieren-Bilder" of Prof. Wachsmüth prove that the jet never vibrates in the simple parabolic manner which he assumes, nor does an antinode lie at the mouth of the pipe. R. König showed that in all well constructed open pipes the operation of blowing causes the central node to be depressed by about one fourteenth of the speaking length, causing an appreciable variation of pressure at the mouth.

V. Lough gives too few particulars of his pipes for his results to be of use. The work of S. Bhargava and R. N. Thorp, however, is fairly well detailed, and serves to support the more recent theories of the organ pipe.

The theory given in most treatises on acoustics, concerning the production and maintenance of sound in flue pipes, is incorrect, and Hermann Smith, Cavaillé-Coll, Dr. Audsley and other famous organ-builders have attempted in vain to correct statements which are still upheld by the authority of such names as Helmholtz, Rayleigh, Tyndall and Deschanel.

III.—ON THE TEMPERATURE GRADIENT IN GASES AT VARIOUS PRESSURES.

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(Assistant Lecturers in the University College of Wales, Aberystwyth.)

Received June 26, 1924.

(*Communicated by* PROF. GWILYM OWEN.)

ABSTRACT.

A careful re-examination of the problem of the temperature gradients through gases at various pressures has been attempted.

Sets of curves indicating these gradients at various pressures and other interesting relations were obtained. Amongst other features, they show quite definitely the existence in contact with the vessel walls—at low pressures—of very large " temperature jumps."

An attempt to explain the general character of the results is based on the assumption of the presence at the walls, of a gas film possessing certain properties due to the attracting forces existing amongst wall molecules and gas molecules.

Instead of using one thermocouple only to determine the temperature at any point in the enclosure, several thermocouples of differing thicknesses—and therefore differing curvature—were employed under identical conditions. Systematic differences amongst corresponding readings of the thermocouples were noticed. These may also be attributed to the presence round the various thermocouples of gas films whose thickness and character vary with the thickness of the couple.

INTRODUCTION.

EARLY experiments on the heat conductivity of gases showed that below a certain small pressure the conductivity no longer remained constant, but diminished with decreasing pressure. Subsequent experiments by Smoluchowski* indicated that this diminution could not be explained on the assumption that the coefficient of conductivity of the gas itself varied with pressure ; for, in the case of a thin layer of gas enclosed between two solid surfaces maintained at different temperatures, the effect was found to depend on the thickness of the layer. The obvious conclusion was that the temperature gradient through the gas was not constant.

Smoluchowski suggested that this variation of the conductivity with pressure might be explained if one assumed a discontinuity of temperature at the surface separating solid and gas. The existence of such a discontinuity had previously been predicted on theoretical grounds by Kundt and Warburg.† The phenomenon was supposed to be analogous to the well-known " slip " effect in viscosity ; an effect first discussed by Helmholtz for liquids and observed by Kundt and Warburg for gases.

In the case of viscous slip in a gas, the equation representing the state of affairs at the boundary between solid and gas can be expressed thus :

$$\xi\frac{dv}{dn}=v-v'$$

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* Wiedemann's Annalen, 64, p. 101 (1898).

† Poggendorf Annalen, 156, p. 177 (1875).

where v =velocity of solid surface.

v' =velocity of fluid at surface.

dv/dn=velocity gradient along normal to solid surface.

and ξ is the coefficient of slip.

In the case of a gas, ξ is found to depend on the mean free path, λ, of the gas molecule. It may therefore be written $=k\lambda$, where k is a constant depending upon the nature of the gas and surface.

Clearly ξ may also be written $=\frac{b\,760}{p}$, where p is the gas pressure in mm.

Analogously—according to Smoluchowski—we have in the case of heat conduction in a gas

$$\gamma\frac{d\theta}{dn}=\theta-\theta' \quad \ldots\ldots\ldots\ldots \quad (1)$$

and

$$\gamma=k'\lambda=\frac{a\,760}{p} \quad \ldots\ldots\ldots\ldots \quad (2)$$

The quantity γ is the so-called "temperature jump coefficient," and corresponds to the coefficient of slip in viscosity.

The meaning of equation (1) will perhaps be made more clear by Fig. 1.

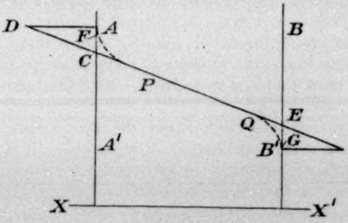

FIG. 1.

AA' and BB' represent the traces of two plane surfaces maintained at temperatures θ_1 and θ_2 ($\theta_1 > \theta_2$), enclosing a quantity of gas. If the heights above XX' represent the temperatures of the plates or gas above some arbitrary temperature, it is clear that the temperature distribution through the gas will be represented by a line on the diagram.

Let the point A represent the temperature θ_1, and B' the temperature θ_2. Then in the simplest case of a uniform gradient through the gas with temperature discontinuities at the plates, the distribution of temperature will be represented by some line $CPQE$. In this case, the slope of PQ—i.e., of CD—is a measure of $d\theta/dn$;

the length AC is a measure of the temperature jump $\theta_1-\theta'$; and AD is a measure of the coefficient γ.

Several investigations have been carried out with a view to testing this theory relating to the discontinuity of temperature between a solid and a gas ; and, in some cases, to finding the value of the product γp, or virtually of the constant a of equation (2).

Amongst the investigators may be mentioned Kundt and Warburg,* Smoluchowski,† Knudsen,‡ and Gehrcke.§ The most direct method, however, was that used by Lasareff,‖ who attempted to measure the actual temperature gradient through a gas layer contained between two parallel metal plates maintained at different temperatures.

The temperature differences employed by him—as by most of the workers previous to him—were small, the largest being about 20°C. Moreover, the temperature of the metal surfaces in contact with the gas were assumed to be those of the oil baths used to heat or cool them.

Of recent years, the experiments of Langmuir¶ and others have brought into prominence the view that a solid surface is in general covered with a film of adhering gas molecules. It is natural to suppose that the temperature gradient in the neighbourhood of the plates in the case referred to above will be affected by the presence of such a film. If this be so, the temperature distribution between the plates in Fig. 1 will not be represented by the straight line $CPQE$, but more probably by some curve $FPQG$, where F and G are points between C and A and E and B' respectively. The discontinuity or temperature jump between gas and plate in the simple case first considered and represented by AC, will now refer to the gas *proper* and the plate ; we may regard the gas *film* as acting somewhat in the manner of a transition layer.

It seemed probable that information on these various points might be obtained by a study of the temperature gradients through different gases in an unequally heated enclosure at different pressures. Such a study might also yield information as to the properties and character of the gas films when present. It was primarily with these objects in view that the experiments described in this Paper were undertaken.

Assuming a connection to exist between the above-mentioned gas film and the temperature jump in the neighbourhood of a solid surface, it is clear that the problem to be studied may become somewhat complicated ; for such gas films must also be present at the surfaces of the thermometers used to investigate the temperature gradient—in our case thin wire thermocouples—and difficulties may arise as to the correct interpretation of the various indications. Further, it is not unreasonable to suppose that, in addition to such obvious possible factors as the nature and temperature of the gas and solid, and the pressure of the gas, the properties of the film may to some extent depend upon the curvature of the solid surface. This possible complication led us to employ for the examination of the temperature gradient several thermocouples of different thicknesses instead of one only.

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* Poggendorf Annalen, 156, p. 177 (1875).

† Wiedemann's Annalen, 64, p. 101 (1898).

‡ Annalen der Physik, 34, p. 593 (1911).

§ Annalen der Physik, 2, p. 102 (1900).

‖ Annalen der Physik, 37, p. 233 (1912).

¶ Physical Review, 43, p. 149 (1916).

By reapproaching the whole problem along the above lines some rather interesting results have been obtained. Those relating particularly to the temperature gradient through a gas are dealt with in this Paper, whilst those relating to the effect of curvature on the indications of a thermocouple will be treated more fully in a subsequent Paper.

APPARATUS.

Fig. 2 represents diagrammatically a section of the chief portion of the apparatus.

A cylindrical brass tube of diameter 12 cm. and of length 3·9 cm., was supported with its axis vertical. The ends of the tube were closed by two carefully turned and machine-faced brass discs of larger diameter, 18 cm., and of thickness 0·5 cm.

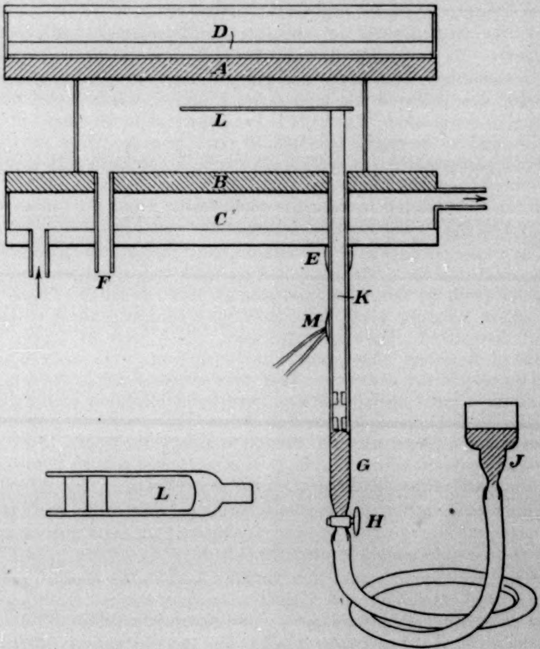

FIG. 2.

The upper disc *A*, referred to subsequently as the "hot plate," formed the lower surface of a brass box which contained an electrically heated coil *D*. This coil—a flat spiral of nichrome wire—covered the whole of the plate *A*, and thus, extending beyond the circular walls of the test-chamber, ensured a uniform temperature over that part of *A* which formed a wall to the enclosure. The heating coil was supplied from a 100-volt battery; it was seldom found necessary to adjust the current after an experiment had begun, the temperature of the hot plate remaining very steady for several hours at a time.

The lower plate *B*—the "cold plate"—formed part of a brass tank *C*, through which a steady and controllable stream of cold water flowing under constant head was passed. As in the case of the heating arrangement, and for a similar purpose, the cooling apparatus extended beyond the circular walls of the test chamber. In this case also the temperature of the cold plate usually remained constant throughout an experiment.

Into two small holes cut in the cold plate, on opposite sides of the centre and near the edge, were soldered two brass tubes *E* and *F*, which passed through the water tank *C* to the outside. The tube *F* was connected to the supply of gas under investigation, and to the pumps and McLeod pressure gauge. The vertical tube *E* was connected permanently to a vertical glass tube *G*, furnished with a stopcock *H*, which permitted communication with a mercury reservoir *J*, whose height could be varied. It was thus possible by suitably manipulating *J* and *H* to adjust delicately the mercury level in *G* to any required height.

Floating in the mercury in *G* was a thin quartz tube *K*, which penetrated into the experimental chamber. The upper end carried a very thin horizontal porcelain platform *L*, of which the dimensions and plan are given in inset to Fig. 2. On this platform, and in the same plane, were mounted three independent thermocouples of different diameters.* These thermocouples, which were of copper and constantan, and of diameters 0·0044, 0·0126 and 0·0209 cm., were made so as to have practically no blob at the junctions. They were stretched across the arms of *L*, as in Fig. 2, about 5 mm.s apart, and were fixed to the platform with an insulating cement; their general appearance was that of three almost accurate coplanar parallel cylinders of common length 1·2 cm. The wires, except the length just mentioned, were completely covered with a thin layer of cement, and passing down the quartz tube left the apparatus through a hole *M* in the glass tube *G*.

In addition to the above thermocouples, two more were used; one was soldered to the hot plate, the other to the cold plate, at about 1·5 cm. from their centres. The wires from these couples passed through the tube *F* to the outside.

Before the five thermocouples were actually fixed in the apparatus, they were roughly calibrated relatively, and against a mercury-in-glass thermometer in a black-body enclosure. They were again more accurately calibrated in the same enclosure at the end of the experiments. The fact that the various results obtained over many months show no variations whatever, justifies the adoption of this final calibration for application to the whole series of results; particularly to those given in this Paper, which were obtained within one month of such calibration.

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* Owing to an accident later, only two of these three couples were available. These are referred to in the text as the *thin* (0·0126 cm.) and the *thick* (0·0209 cm.) couples respectively.

The method of determining the distance of the plane of the platform thermocouples from the hot and cold plates was as follows :—

TABLE I.

Air in chamber. Pressure : atmospheric. Hot plate temperature =179°C. Cold plate temperature = 49·5°C.		Results for thin thermocouple. Pressure=0·0015 mm. Hot plate temperature =179·3°C. Cold plate temperature = 49·0°C.	
Distance from *H*.	Temperature.	Distance from *H*.	Temperature.
mm.	°C.	mm.	°C.
1·00	163·3	1·00	108·0
1·95	152·2	2·50	102·7
2·95	143·2	3·50	101·4
5·05	131·5	7·50	98·2
7·20	125·1	15·55	95·6
13·75	109·5	26·60	90·0
23·35	93·5	31·25	87·3
33·50	76·3	33·00	86·3
35·50	70·1	35·50	85·0
37·05	63·0	37·00	83·3
37·90	57·0		

TABLE II.

Hydrogen in chamber. Pressure : atmospheric. Hot plate temperature =171·5°C. Cold plate temperature = 47·7°C.		Results for thin thermocouple. Pressure=0·0027 mm. Temperature of H=171·2°C. Temperature of C = 48·5°C.	
Distance from *H*.	Temperature.	Distance from *H*.	Temperature.
mm.	°C.	mm.	°C.
1·00	164·7	1·00	111·8
1·80	160·4	2·00	109·7
2·80	155·9	3·15	107·5
4·35	149·2	5·80	106·4
7·25	138·2	13·00	103·9
9·80	129·9	20·80	101·8
15·05	115·5	27·05	100·0
19·65	103·4	34·10	98·2
26·65	85·0	36·00	96·7
29·80	76·5	36·90	95·7
33·55	67·0		
35·00	62·7		
35·80	59·2		
37·00	56·0		

Before the cylinder and hot plate (previously soldered together) were placed in position on the cold plate, the height of the thermocouples above the centre of the cold plate was found by means of a cathetometer by observing the heights of these couples above a series of points on the rim of the cold plate which was horizontal. At the same time, by means of a vertically travelling microscope, the height of a fiducial line on the quartz tube as seen through the glass tube *G* was found. The microscope readings were thus calibrated against those of the cathetometer. In the same way, when the hot plate and cylinder had been soldered into position, observations by the cathetometer on points of the rim of A gave the height of the centre of the hot plate. The error in the estimation of the distances of the couples from the two plates is probably not greater than 0·1 mm.

In the absence of a more convenient apparatus, the following potentiometer arrangement was used to measure the E.M.F.s of the thermocouples :—

E is an accumulator.
W is a standard cadmium cell (1·0184 volts).
M is a variable resistance.
G is a low resistance galvanometer.
SR is a subdivided megohm to the mid-point of which the cell W is connected.
P, Q are two calibrated resistance boxes.

The resistance $P+Q$ was kept constant and equal to 9,200 ohms, whence it follows that for no deflection of the galvanometer G, when the points a and c or b and c are joined, the resistance in ohms taken from the box Q is equal numerically to one-half the E.M.F. of the thermocouple under test in microvolts. A common cold couple was kept at room temperature in a water bath.

With this arrangement, when dealing with temperatures giving E.M.F.s of the order of 8,000 microvolts, readable deflections were obtained for changes of four microvolts. The manipulation of the resistances was somewhat laborious, but the results obtained were very consistent.

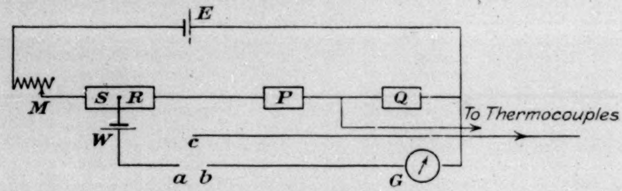


FIG. 2A.

It will be seen from the foregoing description, that we have in effect a layer of gas some four centimetres thick, enclosed between two horizontal plates—maintained at different temperatures—under conditions in which convection, and the influence of the sides on the main body of the gas may be assumed negligible ; and in which the lines of flow of heat—at any rate through the central portion of the layer—may be assumed vertical. That these assumptions were justified was shown by swinging the thermocouples round the axis of the quartz tube in a horizontal plane when the temperatures indicated were found to be constant.

METHOD OF CARRYING OUT AN EXPERIMENT.

In an experiment, say, with air, the apparatus was many times alternately exhausted, and filled with air dried by passing over calcium chloride and phosphorus pentoxide. Finally, the apparatus was filled with dry air at the desired pressure.

During this time, the hot and cold plates were taking up the temperatures chosen. Depending upon the circumstances, usually from three to four hours elapsed before conditions were steady, and observations could be commenced.

The platform thermocouples which had previously been placed as close to the hot plate as possible, were then gradually moved downwards by measured steps, until the cold plate was reached. At each step, the E.M.F.s registered by all the thermocouples, including those on the hot and cold plates, were noted. These observations usually took about two or three hours, since for each movement of the platform, it was necessary to await steady conditions.

For the sake of uniformity and ease of comparison, the experiments described in this Paper were carried out with the hot and cold plates at constant temperatures of about 180°C. and 50°C. respectively.

RESULTS.

With the exception of those given in the Appendix, the results in this Paper deal with the general problem of the temperature gradient through a gas.

The results given in the Appendix show that under certain conditions—e.g., in air at atmospheric pressure—there is a systematic difference between the readings of the thick and thin thermocouples. Presumably on all grounds the results obtained with a very thin thermocouple will most nearly represent the true state of affairs in the gas chamber. For this reason, and for the sake of uniformity, the graphs and calculations have been based on the indications of the thinner thermocouple, although of course, readings were taken for both.

An examination of the results given in the Appendix will show that the greatest difference between the readings of the two couples was only about 1½°C. Thus, although it is possible that with still thinner thermocouples the difference might be larger it is probable that the present results represent with a fair degree of accuracy the temperature distribution through the gas.

In the tables the temperatures are expressed in degrees centigrade. They have been deduced from the calibration curve which was obtained, as previously explained, in the black body enclosure.

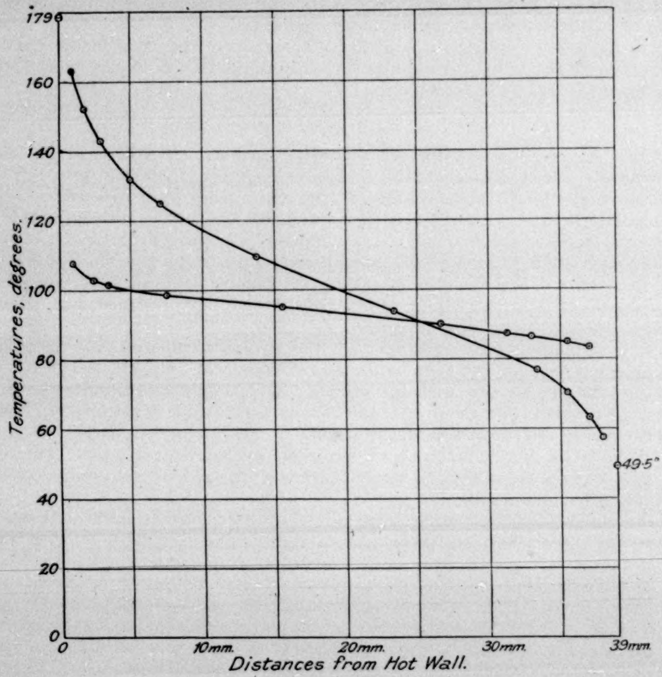
The graphs have been constructed directly from the tables given. It was not possible for the platform thermocouples to approach the plates more closely than 1 mm., and no attempt has been made to extrapolate the experimental curves. It may be observed, however, that in the curves for very low pressures (*see* Graphs 1 and 2) those curves appear, if produced, to cut into the plates at points not coincident with those representing the temperatures of the plates ; thus indicating the existence of a discontinuity of temperature at the plate in addition to a variable gradient in the neighbourhood. This is not noticed in the case of high pressures.

The upper curve in Graph 1 shows the temperature distribution across the chamber with air at atmospheric pressure. The hot plate (H) had a temperature of 179°C., and the cold plate (C) one of 49°C. It is seen that the temperature distribution is not uniform. In the first 5 mm. from H, the temperature falls 46°C., in the last 5 mm. it falls 26°C., whilst across the remaining 29 mm. the drop is only 58°C.

The form and general dimensions of the chamber ensure that approximately the same quantity of heat per second crosses every central horizontal section of the chamber—i.e., $dQ/dt=Kd\theta/dn=$constant, where K is the conductivity coefficient, and $d\theta/dn$ is the temperature gradient along the vertical axis. The results therefore appear to indicate a variation in the conductivity coefficient K in the regions near H

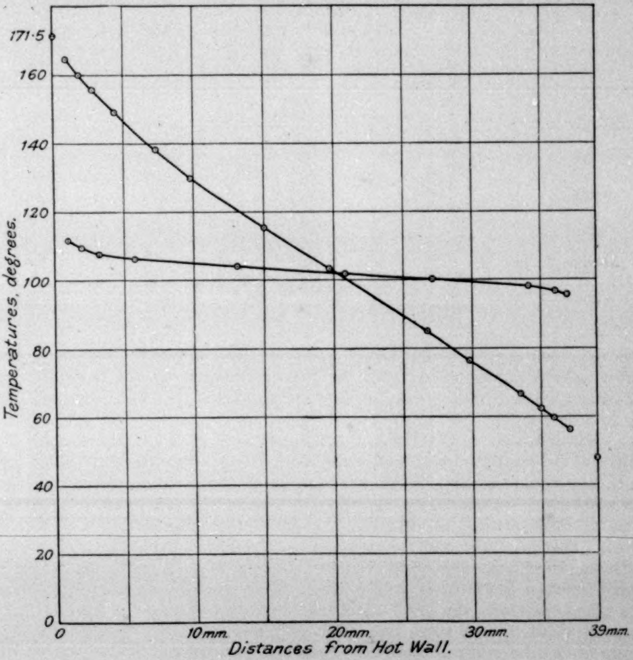
and *C* ; which suggests the presence of some sort of transition layer of air near the plates.

The lower curve in Graph 1 gives the temperature distribution with air at a pressure of 0·0015 mm., the temperatures of the plates being the same as for the upper curve. We now have a small uniform gradient through the bulk of the gas (hereafter referred to as the ***bulk gradient***), with large jumps at the ends. At a distance of 1 mm. from the hot plate, there is a fall of 71°C. If the straight portion of the curve be produced both ways to meet the plates *H* and *C*, we get temperature jumps of 77°C. at *H* and 35°C. at *C*, leaving only 18°C. for the fall across the chamber. It is clear that the character of the transition layer is in some way different from that present at atmospheric pressure.



GRAPH 1.

Graph 2 indicates the results when hydrogen from a gas cylinder replaced the air in the chamber. The upper curve refers to hydrogen at atmospheric pressure, the lower to the same gas at a pressure of 0·0027 mm. Although both curves are of similar type to those in Graph 1 for air, certain differences are to be noticed. In the upper hydrogen curve, the bulk gradient is seen to be more nearly uniform and steeper than in the corresponding air curve. Thus, in the first 5 mm. the temperature drop is only 24°C., in the last 5 mm. it is 18°C., whilst across the remaining 29 mm. it is 81°C. In the lower hydrogen curve, the bulk gradient is uniform, but less steep than in the corresponding air curve. Further, whilst in the case of air at low pressure the temperature jumps at the two plates are very much different, the corresponding jumps in the case of hydrogen are much more nearly alike. The same effect, but less noticeable, can be found in the upper curves of Graphs 1 and 2.



GRAPH 2.

The comparison may be summed up in the statement that for the same temperatures of hot and cold plates, the bulk gradient in hydrogen for various pressures varies over wider limits than that in air, whilst the general temperature distribution between the plates is more nearly symmetrical in the case of hydrogen.

Table III was compiled in a somewhat different manner from the preceding tables.

TABLE III.—*Air.*

Temperature of hot plate = 173·7°C.
Temperature of cold plate = 49·0°C.

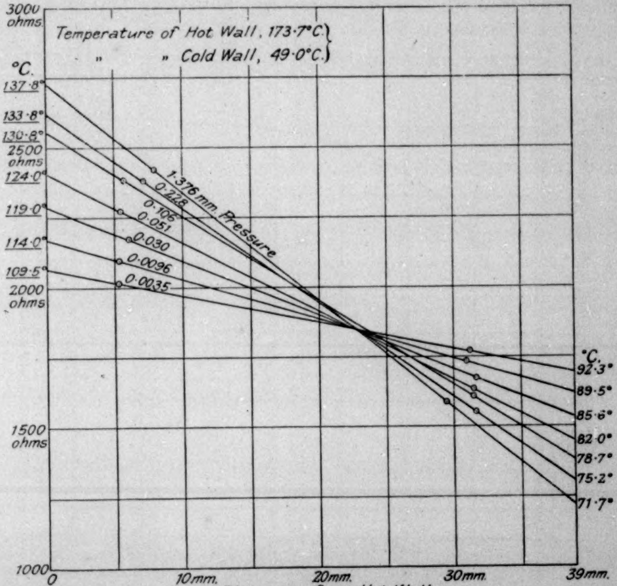
Pressure.	Distance from hot plate.	Temperature in terms of ohms out of box Q.	Temperatures at H and C in deg. C. on producing lines in Graph 3 to meet plates.	
mm.	mm.	ohms.	θ'_H.	θ'_C.
1·376	7·85	2,427	137·8	71·7
	29·60	1,587		
0·228	7·00	2,388	133·8	75·2
	31·40	1,557		
0·106	5·60	2,390	130·8	78·7
	31·25	1,613		
0·051	5·40	2,274	124·0	82·0
	31·25	1,634		
0·030	5·90	2,176	119·0	85·6
	31·80	1,683		
0·0096	5·30	2,096	114·0	89·5
	30·75	1,739		
0·0035	5·30	2,019	109·5	92·3
	31·00	1,768		

Several curves at different low pressures in air—of which only one is given in Graph 1—showed quite clearly that the bulk gradient remained perfectly uniform until within about 3 mm. of the hot and cold plates. This fact suggested that, in order to determine the bulk gradients, and hence the value of $d\theta/dn$ at low pressures, it was only necessary to determine the temperatures of the thermocouples for two positions of the platform, one about 4 mm. from H, the other about the same distance from C. It would thus be possible to obtain in a comparatively short time—several hours instead of several days—and therefore under the same conditions, the bulk gradients corresponding to different low pressures—information of considerable interest. This was done, and the results are embodied in Table III. and in Graph 3.

The maximum possible bulk gradient is clearly $(\theta_H - \theta_C)/HC$, where HC is the distance between the hot and cold plates. Although from very low pressures and up to about 0·06 mm. the gradient is seen rapidly to increase, there is but little change onwards to 0·2 mm., and from 1·5 mm. to atmospheric—the highest pressure used—the gradient appears to remain stationary.

The fact that all the lines pass through one point suggests that whatever the actual temperature jumps at the plates may be, the *ratio* of the hot "jump" to the cold "jump" for fixed temperatures of the two plates is independent of the pressure. This ratio is that of the distances of the point of intersection of the lines from the two plates, and is seen to be as 45½ to 32½. It is interesting to notice that this ratio is the same as that of the absolute temperatures of the hot and cold plates, viz., 447 to 322. Unless this be a curious coincidence, it suggests that the temperature jumps at the plates for air for any fixed pressure are proportional to their absolute temperatures.

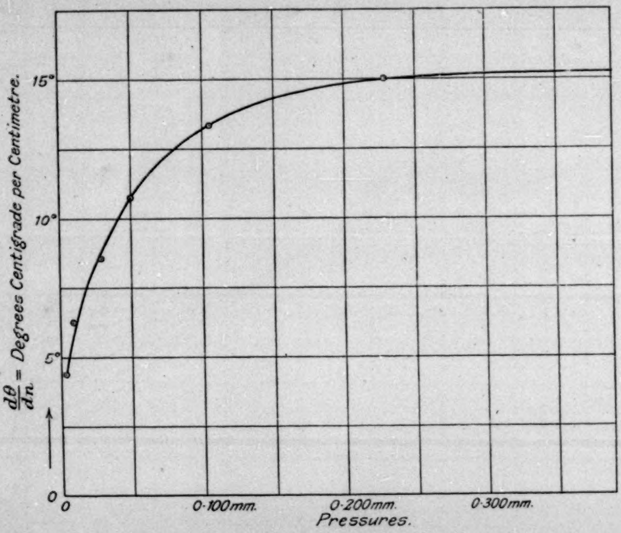
The variation of the bulk gradient—the $d\theta/dn$ of equation (1)—with pressure is well brought out in Graph 4, which was constructed from the same data as Graph 3. The curve stops at the point corresponding to the gradient for the lowest pressure used, and no attempt has been made to estimate the non-experimental portion. Above 0·06 mm., $d\theta/dn$ slowly increases with pressure, apparently reaching a final and maximum value at about 1·5 mm. Below 0·06 mm., $d\theta/dn$ diminishes very rapidly indeed, and the curve, if produced, would apparently pass through the origin. Such an occurrence could only indicate one of two improbable things. Either the gradient in a perfect vacuum under the above conditions is zero—a difficult fact to conceive if the couples receive any heat due to radiation—or the radiating surfaces are not the metal plates, but some other surfaces. In the latter case, we may assume them to be the outer surfaces of the gas films adhering to the plates, and must then assume the temperature drops through these films to be such that these two surfaces have equal temperatures. We can be almost certain, therefore, that the curve cuts the gradient axis at some point near to, but not coincident with, the origin, indicating that though finite, the effect due to radiation is very small.



GRAPH 3.

It is interesting to examine Graph 4 in conjunction with some results obtained by Soddy and Berry* when investigating the heat losses from a hot wire through gases at different pressures. They obtained for the variation of the heat loss per second with pressure a curve almost identical with the one given in Graph 4. It is clear that in the equation $dQ/dt = K d\theta/dn$, Soddy and Berry found the variation of the quantity dQ/dt with pressure, whilst the experiments described in this Paper give the variation of $d\theta/dn$ with pressure.

It is true that the two sets of experiments were carried out under very different conditions; but if we assume for the moment that the variation of the bulk gradient is the same in both cases, we see that the quantity K is really a constant. In any case, a method of experimentally finding the variation of K with pressure is suggested; for it is simply required to investigate at one and the same time, i.e., under the same conditions, the variation with pressure of both dQ/dt and $d\theta/dn$. The apparatus used in this Paper is inconvenient and ill adapted for such an experiment, but one similar to that used by Soddy and Berry would, with the necessary modifications, be very suitable.



GRAPH 4.

Comparing once more Graph 3 with Fig. 1, it is seen that the $\theta_H - \theta^1_H$ of equation (1)—which is the temperature jump at the hot plate—may be obtained by producing back the lines to meet that plate. Incidentally, the intersection of the same lines with the cold plate serve to determine $\theta_C - \theta^1_C$. The results of such operations are collected in columns 3 and 4 of Table IV. From the data of columns 2 and 3 it is now possible to determine γ_H as defined in equation (1). For convenience, the values of $1/\gamma_H$ and $1/\gamma_C$ are given in the table.

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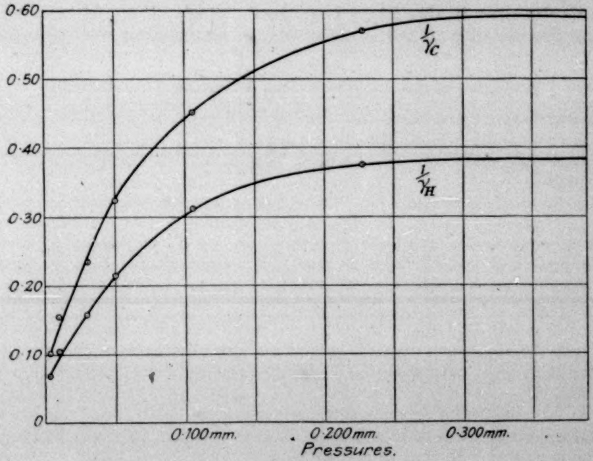
* Proc. Roy. Soc., A., 83, Dec. (1909), Nov. (1910).

TABLE IV.—*Air.*

Pressure.	$d\theta/dn$.	$\theta_H - \theta^1_H$	$\theta_C - \theta^1_C$	$\frac{1}{\gamma_H} = \frac{d\theta/dn}{\theta_H - \theta^1_H}$	$a_H = \frac{p\gamma_H}{760}$	$\frac{1}{\gamma_C}$	a_C.
mm.	°C. per cm.	°C.					
1·376	16·95	35·9	22·7	0·472	0·003830	0·747	0·00242
1·228	15·00	39·9	26·2	0·376	0·000798	0·572	0·000524
1·106	13·36	42·9	29·7	0·311	0·000448	0·450	0·000310
0·051	10·77	49·7	33·0	0·217	0·000310	0·326	0·000206
0·030	8·56	54·7	36·6	0·156	0·000252	0·234	0·000169
0·0096	6·28	59·7	40·5	0·105	0·000120	0·157	0·000080
0·0035	4·41	64·2	43·3	0·069	0·000067	0·102	0·000045

The lower curve in Graph 5 shows the variation of $1/\gamma_H$ with pressure. According to equation (2), this graph should be a straight line. The graph convincingly indicates, however, that $1/\gamma$ is not a linear function of the pressure; indeed, an equation of an exponential form would appear more nearly to express the results.



GRAPH 5.

The same type of curve (the upper one in Graph 5) is obtained for the variation of $1/\gamma_C$ with pressure, as is to be expected.

These curves are very instructive; they seem to indicate that the analogy between the temperature jump effect in conduction and the slipping effect in viscosity is not strictly true. It is, however, quite possible that the velocity gradient in the neighbourhood of a solid surface in the case of viscosity would, if examined by a direct method, show a similar variation with pressure.

It will be seen from column 6 of Table IV. that the quantity a diminishes with decreasing pressure; this is otherwise obvious from Graph 5. The values obtained by Smoluchowski, who believed a to be really constant, was 0·000017 for air in contact with a glass surface.[*] Lasareff,[†] working with nickel surfaces and air, obtained values ranging from 0·000013 to 0·00002. The latter's results show some irregularity, some of the differences being of the order of 40 per cent. of the mean value adopted On the other hand, it will be noticed that the values of a given in Table IV. show a *systematic* variation. It will be further noticed that as the pressure decreases, the value more nearly approaches that of other workers, and an examination of Graph 5 shows that their values will ultimately be reached at sufficiently low pressures. By comparing a_H and a_C, as given in columns 6 and 8 of Table IV, it appears that the value of a diminishes with temperature. In this connection it is interesting to notice that Lasareff and most of the previous investigators worked with temperatures of the order of 15°C. This may assist in explaining the differences in the results given in this Paper from those quoted. It would be interesting to examine the variation of the quantity a with temperature when the pressure is fixed.

GENERAL CONSIDERATIONS AND DISCUSSION OF RESULTS.

Consider an experimental chamber of ideal type, in which the hot and cold plates are two infinite plane parallel surfaces H and C respectively, maintained at temperatures θ_H and θ_C $(\theta_H > \theta_C)$; and suppose at first that no gas films are present at the plates.

Case 1.—Let the pressure be so small that only a few molecules are present, and no mutual collisions occur. In this case, assuming a perfect interchange of energy at the plate, molecules will leave H with an energy E_H corresponding to θ_H. The same number will leave C with an energy E_C corresponding to a temperature θ_C. Now, since there are no mutual collisions between the molecules, it is clear that the mean energy of the molecules at any point of the chamber between H and C will be constant and $=\frac{1}{2}(E_H+E_C)$. If, therefore, a suitable thermometer could be introduced into the chamber, it would—neglecting any effects due to radiation—register a temperature $\frac{1}{2}(\theta_H+\theta_C)$ along HC. In this case we should have a zero gradient, as in Fig. 3.

The fact that even at relatively high pressures (0·001 mm.) the experimental gradient approaches this in type (*see* Graph 1) shows that the effect due to radiation must be very small, so that in the above case the gradient would differ very little from zero.

Further, we see that although the gradient through the gas may approach zero, a definite amount of heat has been transferred from H to C. It is obvious, and to be expected, that the usual relation for heat conduction $\frac{dQ}{dt}=K\frac{d\theta}{dn}$ cannot now hold. We must now regard the heat as transferred rather by a sort of convection than by conduction, as usually understood. Under these circumstances the state of affairs would be represented by an equation of form

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[*] Wiedemann's Annalen, 64, p. 101 (1898).

[†] Annalen der Physik, 37, p. 233 (1912).

$$\frac{dQ}{dt}=K^1 n\,(\theta_{\mathrm{H}}-\theta_{\mathrm{C}}),$$

where n is the number of molecules per unit volume, and K^1 is some constant for the particular gas under consideration.

Case 2.—Let now the gas pressure be increased so that very frequent collisions occur between the molecules. It is clear that if we consider a region near H, although the molecules leaving H will possess a mean energy E_{H} as before, those approaching H will not have come from C; they will have suffered many collisions, and in consequence statistically their energy will be greater than E_{C}. Thus the mean energy of the molecules in the region considered will correspond to a temperature $>\frac{1}{2}(\theta_{\mathrm{H}}+\theta_{\mathrm{C}})$, the degree of inequality depending upon the distance from H. Similarly, near C, the temperature will be $<\frac{1}{2}(\theta_{\mathrm{H}}+\theta_{\mathrm{C}})$. We thus now get a finite temperature gradient along HC, the magnitude depending upon the pressure.

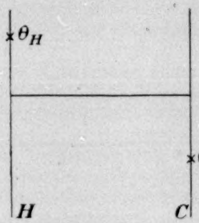

FIG. 3.

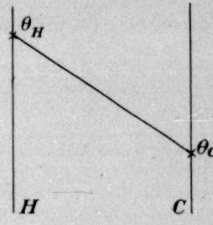

FIG. 4.

The character of the transfer of heat will, when the principles of the ordinary kinetic theory apply, be represented by the usual equation

$$\frac{dQ}{dt}=K\frac{d\theta}{dn}$$

Between Cases 1 and 2 there will, of course, be a transition stage, during which it becomes difficult to associate the coefficient of heat transfer with the coefficient of conductivity as usually defined.

Such elementary considerations would lead to the conception of a gradient somewhat similar to that in Fig. 4, when the pressure is such that the mean free path of the gas molecule is exceedingly small compared with HC, the distance between the plates.

The experimental gradients at pressures satisfying this condition, however, are distinctly different (cf. Graphs 1 and 2). A further study of Graphs 1 and 2 shows that the temperature jumps at the two plates are not the same.

Obviously, therefore, we are dealing with some phenomenon which is peculiar to the region at the wall. It is therefore necessary to examine the mechanism by which the heat transfer between wall and gas occurs. To attempt an analytical discussion of an exceedingly difficult and complicated problem in a Paper of this kind, where the experimental data are ill adapted for numerical verification, would be out of place; the object in this Paper is merely to suggest in a general manner one explanation of the results. The treatment is inspired by a very suggestive Paper by Langmuir,[*] in which, in addition to a summary of previous attempts to explain what occurs at the surface of contact between a plate and a gas, he gives a detailed discussion of the general problem.

Proceeding along lines suggested by Baule,[†] Langmuir attempts to take into account the attractive and repulsive forces which must undoubtedly exist between the molecules of the plate and gas. By doing so, he is able to account for molecules leaving a hot surface with an energy which may be considerably less than that corresponding to the temperature of the surface. However, the effects of these mutual molecular forces do not appear to have been followed to their logical conclusion.

Consider once more the hot plate H, which may be regarded as composed of molecules arranged in some regular configuration. These molecules will vibrate about their positions of equilibrium with a mean kinetic energy dependent upon θ_H and with a probable amplitude not greater than their diameters. According to Langmuir's analysis, a gas molecule approaching the hot plate is subjected to an attractive force exerted by a group of plate molecules, but is finally repelled by a single molecule only: if such a molecule return to the gas, it will do so with a kinetic energy greater than its former energy, but less than that of the plate molecule. For our purpose, we may say that for a single collision the impact may be regarded as imperfect.

Consider, now, a gas molecule approaching H with an energy E_0, corresponding to some temperature θ_0 where $\theta_0 < \theta_H$. When close to H, it will, as just stated, be subjected to an attractive force exerted by the group of plate molecules near it, and will proceed towards the plate with increasing velocity. On reaching the plate, one of three things may occur.

It may (*a*) strike a plate molecule and be sent back into the gas; (*b*) strike a plate molecule, but not return to the gas immediately; or (*c*) not collide with a surface molecule as in (*a*) and (*b*), but pass through the group and enter the solid.

In Case (*a*) of one collision, the gas molecule will re-enter the gas with an energy E_1 greater than its previous energy, but less than E_H. Further, one may reasonably assume that the direction after impact will depend to some extent upon that before. We may therefore regard such a molecule as having suffered reflection at the plate.

In Case (*b*), the gas molecule may, whilst it remains within the region of molecular attraction, suffer a number of collisions, thus moving irregularly over the surface of the plate; it may also vibrate about some position of temporary equilibrium. It may then re-enter the gas, or it may enter the solid as in Case (*c*). It is clear that such molecules will have a better chance of acquiring an energy E_H than those of type (*a*).

In Case (*c*), the molecule will in the mean suffer so many collisions that it will certainly finally possess energy E_H, corresponding to a temperature θ_H.

When the molecules of the two last types finally return to the gas, although the (*c*) molecules certainly, and the (*b*) molecules possibly, will start from the plate with energy E_H, they will not enter the gas with it; for they will lose energy in overcoming the attractive forces which exist at the surface of the plate. Further, the directions of these molecules after leaving the plate will be quite independent of the previous directions of approach. Such molecules are not therefore reflected, but must be regarded as having first condensed upon, and then evaporated from, the surface.

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[*] Physical Review, 43, p. 149 (1916).

[†] Physical Review, 43, p. 149 (1916).

The general situation at the hot plate may be expressed thus :—

Let E_H=mean energy of plate molecules at temperature θ_H.
E_0=mean energy of incident gas molecules.
E_1=mean energy of escaping gas molecules of type (*a*).
E_2, E_3=mean energy of escaping gas molecules of types (*b*), (*c*).
a, *b*, *c*=respective proportions of molecules of types (*a*), (*b*), (*c*).
F_H=mean energy of all gas molecules at hot plate. It will be a measure of the true temperature at this point.

Then

$$F_H=\frac{E_0+(aE_1+bE_2+cE_3)}{2}$$

The values of *a*, *b*, and *c* will depend upon several considerations even when the pressure and temperature are fixed. Evidence from other sources as to the value of *a* is very conflicting (*see* cases quoted by Langmuir*), but the balance is perhaps in favour of the belief that at low pressures for all gases except hydrogen it is almost zero. In the above equation which refers to the hot plate, E_1, E_2, and E_3 are separately greater than E_0 and less than E_H.

There will be a corresponding equation for the cold plate, viz.,

$$F_C=\frac{E'_0+(a'E'_1+b'E'_2+c'E'_3)}{2}$$

The effects of the various terms will not, however, be in the same direction in both equations; for whilst, for example, E'_1 will have a value intermediate between E'_0 and E_C, the value of E_3 must always be less than E_C. The value of E'_2 may be either $>$ or $<$ E_C, depending upon the circumstances. This may explain the lack of symmetry in the curves of Graphs 1 and 2 to which attention has already been directed.

Finally, whilst E_1 is dependent upon both E_0 and E_H, possibly in the manner discussed by Langmuir, E_3 will be equal to $E_H-\varepsilon$, where ε is the work done by the escaping molecule in overcoming the attractive forces at the plate. The value of E_2 will have some value between E_0 and $E_H-\varepsilon$. The case of the cold plate is obviously similar.

We may regard the three types (*a*), (*b*), and (*c*) as representing the gas molecules which are (*a*) reflected, (*b*) condensed upon and (*c*) absorbed by the surface. Their relative proportions will depend upon a variety of circumstances. Thus, if a gas be gradually introduced into a perfectly evacuated vessel whose walls are originally gas-free, we may be certain that at first a large proportion of the molecules will belong to type (*c*). In fact, we may say that at all pressures and temperatures there is an attempt to saturate the solid and to form a gas layer upon the surface. The effect of increase of pressure must be to assist this action, that of temperature in general to oppose it. When, in the case just quoted, for a given temperature the pressure is such that we may regard the interstices of the solid near the surface as in the mean full, and a film of molecules—at first, perhaps, only one deep—to be on the surface, we shall have a new state of affairs. The number of molecules of type (*c*) will be small, and those of types (*a*) and (*b*) will now collide in general not with the wall molecules, but with the gas molecules in the film. The magnitude of the attractive forces considered above will be different from and less than formerly. Because of this, and also because molecules in the gas film will be less firmly held together than the surface wall molecules themselves, the degree of interchange of energy will be greater.

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* **Physical Review, 43, p. 149 (1916).**

Just as a film of gas molecules forms on the surface of the solid, so a second set of gas molecules will form on the first set, and so on. But it is clear that succeeding sets will be less firmly held together than the preceding ones, with the result that, although as the pressure increases the film will thicken, it will not be uniformly dense, but will merge finally within a short distance into the gas proper. The effect on the degree of interchange of energy in collisions is obvious ; the collisions of gas molecules from the main body of the gas with those in the outermost layer of the film will be almost identical with collisions amongst the free molecules themselves. As the gas layers merge into the gas proper, the conception of a surface disappears, and it is clear that molecules of type (*b*) will gradually become of type (*c*), which, entering the gas film, will move about in very weak fields, and when escaping will only have to do so against very weak attractive forces.

Applying this conception of a permanent gas film at the plates to the experimental results, it is clear that the bulk gradient will depend on the temperatures of the outer layers of the gas films at the two plates, i.e., on the thickness and character of the films.

Graph 4 for air shows that from zero pressure to about 0·06 mm. the bulk gradient $d\theta/dn$ very rapidly increases, but that the *rate* of increase quickly falls off from 0·20 mm. to about 1·5 mm., and appears to be zero above the latter pressure. If the foregoing discussion be sound, we may infer that the first layer of gas molecules on the surface is formed at a pressure lower than 0·06 mm., and that the film has attained its maximum thickness before a pressure of about 1·5 mm.

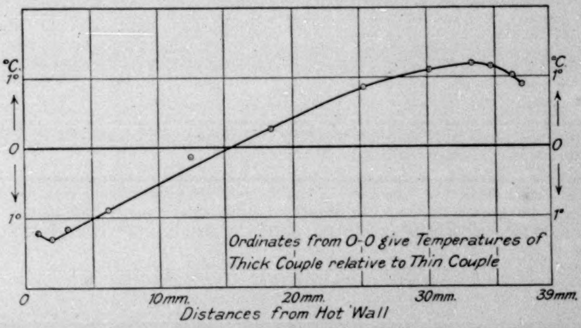
The upper curve in Graph 1 for air at atmospheric pressure shows that the bulk gradient is practically constant until within a distance of a few millimetres of the plates. We may conclude that the gas films for air under these conditions are not greater than a few millimetres.

We may reasonably take the thickness of such a film as a qualitative indication of the degree of departure of the gas considered, from the perfection required by the ordinary kinetic theory, since the gas films get thicker as a result of the mutual attraction between the gas molecules. Thus the more nearly a gas approaches perfection the more nearly will the bulk gradient at high pressures approach the maximum gradient $(\theta_H-\theta_C)\div HC$. A comparison of the upper curves in Graphs 1 and 2 suggests that the mutual attractive forces amongst the hydrogen molecules are much less than in the case of air—a fact well established from other considerations.

APPENDIX.

In the foregoing Paper it was pointed out that although only the results for the thin thermocouple are considered in the tables and graphs, results were also obtained for the thick couple in carrying out the experiments described. It was further pointed out that both thermocouples were calibrated together against a mercury-in-glass thermometer in a black-body enclosure over the whole range of temperature employed. It is clear that if there be no effects due to a difference in curvature of the surfaces there will be no difference in the indications of either couple.

In the actual experiments, however, a small but definite and systematic difference was found to exist between the two couples in air at atmospheric pressure. An example of the results is shown in Graph 6, where the difference in degrees centigrade between the two sets of readings for the two couples is plotted against different distances from the hot plate, when the hot and cold plates were maintained at temperatures 179°C. and 49°C. respectively.



GRAPH 6.

It will be seen that the thick thermocouple indicates a lower temperature than the thin couple in the hotter portion of the chamber, but a higher temperature than the thin one in the colder portion. This interesting phenomenon was not observed in the case of air at low pressures, and was barely perceptible in the case of Hydrogen at any pressure.

In view of the important bearing this matter may have on the determination of temperatures in unequally heated enclosures at ordinary pressures, further experiments are being carried out, and it is hoped shortly to publish a second Paper on the subject.

It is highly probable that the explanation lies in the variation with curvature of the thickness of the gas film which exists at the surfaces of the wire thermocouples.

In conclusion, we wish to express our thanks to Prof. G. A. Schott, who originally drew our attention to the problem of the temperature discontinuity existing at the surface between a solid and a gas. We especially wish to thank Prof. G. Owen for his stimulating interest and advice throughout the course of the experiments.

DISCUSSION.

Mr. B. S. Gossling said that the temperature in a space containing gas at low pressure was one of those delicate and uncomfortable subjects which should be mentioned only in carefully chosen society ; he felt, however, that with the present authors it was in perfectly safe hands. He thought that the authors had dismissed the influence of radiation rather briefly. Radiation might be an important factor at a temperature of 150°C. As regards the case illustrated in Fig. 3, where the absence of molecular collisions was assumed, an exactly similar temperature distribution would be obtained in a perfect vacuum, owing solely to radiation. Must not the authors' curve, therefore, be regarded as determined by radiation and not by conduction ? There is one objection to such a view : it would imply that very near to the hot plate the thermocouple should register a temperature four times closer to the hot than to the cold temperature, assuming the fourth power law of radiation ; whereas in graph 1, for instance, the experimental curve lies nearer to the cold than to the hot temperature. At the end of their theoretical discussion the authors speak of the Langmuir film. The word " film " which has come into use in this connexion seems a misnomer, as it denotes a region 4 mm. thick—viz., that in which convection ceases and conduction only is effective, a discontinuity at the edge of this region being indicated by optical experiments. The authors' discussion of a really thin film is very interesting, but such a film is quite different from the Langmuir layer. The continuation of the authors' work might throw some new light on the nature of the adsorption of gases. An interesting case would arise where the hot plate was incandescent, but the difficulties involved in an experimental investigation of it would be considerable.

Dr. Ezer Griffiths: I think it is a pity that the discussion of the methods of deducing true temperatures from the indications of the thermocouples should have been deferred, for it is difficult to assess the value of the theoretical edifice which has been built up unless one is satisfied as to the experimental foundation. I have not had the opportunity of studying the Paper in detail, but glancing through it I find no reference to two Papers by G. H. Henderson in the Physical Review for 1920, entitled : " A new method of determining the temperature variation of the thermal conductivity of gases," and " The determination of the radiation error in the measurement of gas temperatures." Henderson's method is similar to that of the authors, but his thermocouples were of much finer diameter, 1 mil as compared with the 5 mils of the present Paper. One of Henderson's remarks in his conclusions might be quoted : " One point is clearly brought out in the present work, viz., the unreliability of temperatures as indicated by a temperature-measuring device placed in a gas in the presence of surfaces at temperatures differing considerably from that of the body of the gas." I would suggest that the authors should make a careful intercomparison of their work with that of Henderson, for he seems to have devoted much time to the problem of the measurement of temperature at a point in the gas. Personally I should favour a method of determining the temperature gradient by some interference method utilising the change of refractive index of the gas with temperature. As regards Mr. Gossling's remarks concerning the Langmuir film theory, this has received considerable modification and I would refer him to a Paper by Chester W. Rice in Journ. Amer. Inst. of Elec. Eng., 1923.

Mr. Gossling agreed that the Langmuir theory had been modified, but pointed out that the boundary of the Langmuir region now assumed to divide the turbulent from the streamline motion in the gas, occurs at a definite distance from the solid surface. Interference methods of measuring temperature lead to difficulties at low pressures, where " temperature " ceases to have a useful physical meaning.

Mr. C. R. Darling referred to the discrepancy between the readings of the two thermocouples, and suggested the use of flat strips similar to those employed by Dr. Moll (*see* Proc Phys. Soc., Vol. 35, p. 257). The Langmuir layer need not be an actual layer ; all that it implies is that in the region in question thermal phenomena are the same as they would be if such a layer existed. He had determined the temperature distribution in badly conducting solids, such as magnesia and asbestos, and obtained curves resembling Graph 1, except that they had not been extended towards the outer edge. The curves if extended by extrapolation would indicate a temperature about 15° higher than the actual temperature of the hot wall. In these experiments the specimen was cylindrical and the heat flowed parallel to the axis. The thermocouples were buried, the wires being normal to the axis.

The PRESIDENT suggested that in view of the discussion the authors should add to the Paper a note on their temperature measurements.

AUTHORS' reply (communicated): (In reply to Mr. Gossling). We consider that the question of radiation might with advantage have been given more attention. The temperature gradient due to radiation alone in an apparatus of finite size is probably most satisfactorily examined experimentally. In our case, for example, the radiation gradient to be expected according to Mr. Gossling is not supported by experimental results (*see* Graph 3).

Although the relative effect of radiation on the indications of the thermocouples will increase with decrease of pressure, the question as to whether the curves at our lower pressures are determined by radiation rather than by conduction will depend on the absolute magnitude of the gradient due to radiation alone. In our Paper, we were inclined to consider this gradient almost negligible, especially since the gradient for our lowest pressure, which is still comparatively high, was small.

(In reply to Mr. Darling). The use of flat strips instead of wires for thermocouples might be a useful variation in the conditions, and therefore help to determine the magnitude of the effect of radiation.

(In reply to Dr. Griffiths). The question reduces to that of the efficiency of a thermocouple for the measurement of temperatures under the conditions of the experiment.

We are grateful to him for referring us to the Papers by G. H. Henderson, the more so since the latter refers to some previous work by Kreisinger and Barkley. A critical comparison of the Papers as suggested would, to be satisfactory, require a further Paper. It might, however, be pointed out that whilst we agree with Henderson's general analysis of the problem which deals with atmospheric pressure only, the assumptions made by him in deducing the numerical values of the corrections due to the radiation effect to be applied to the reading of the thermocouple seem too big to warrant much reliance being placed on those values when obtained. Further, the assumption of no discontinuity at the wall as a check on his calculations is hardly permissible.

It will be clear from the results in the Appendix that we had realised that the temperatures indicated by the platform thermocouples at any point could not be the actual temperature of the gas there; and that the thinner the couple, the less pronounced would be the temperature drops at the plates at atmospheric pressure. In view, however, of the small differences found to exist between the two couples, we assumed that the temperatures indicated were probably not far removed from the true values, although at the same time we stated our intention to inquire more closely into the matter. In this connexion, it is unfortunate that we were not able to use our third and thinnest platform couple, which was of similar diameter to that used by Henderson, especially since a consideration of his work and that of Kreisinger and Barkley inclines us to the view that the departures from the true temperatures are probably greater than we at first anticipated. This will, of course, cause the bulk gradient at atmospheric pressure to be steeper than that given in Graph 1. However, even if the corrections deduced by Henderson for bigger temperature gradients be applied to our results, there will still be a discontinuity at the plates at atmospheric pressure.

Although not raised in the discussion, there still remains the question of the possible effect of the side walls. We feel that this matter deserves some further consideration. One of us is at present continuing the general problem, using an apparatus of type similar to that described in our Paper, but of modified dimensions. Several thermocouples of varying diameters are being employed, and it is proposed to deduce by a graphical method the temperature for an infinitely thin couple. The other proposes studying the problem by a method involving the use of the principles of resistance thermometry. In this case the apparatus will necessarily be of different design, the source of heat being a hot wire. It may be possible with this apparatus to investigate the problem when the wire is incandescent as suggested by Mr. Gossling.

IV.—THE DIELECTRIC PROPERTIES OF MICA.

An Investigation carried out for the British Electrical and Allied Industries Research Association.

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Received October 3, 1924.

Abstract.

Two methods used for determining the dielectric constant of thin sheets of mica are described and the errors discussed.

The results of measurements on over 170 sheets of mica are given. The samples tested included best clear mica, ruby, green and brown in colour, stained and spotted samples, and also samples with gaseous inclusions. There were also samples of amber mica and silver amber mica.

The results show that practically all samples of muscovite (potassium mica) have the same dielectric constant, the most probable value being 7·0. The presence of inclusions has no great effect on this value. The samples of phlogopite (magnesium mica)—i.e., the amber micas—have rather lower dielectric constants, about 6·0 for normal amber mica, and 5·0 for the silver amber variety.

Values of power factor are given for many of the samples. For the clear micas (muscovite) it may be as low as 0·0003, and possibly less. The effect of inclusions is to increase it considerably. The power factor of the amber micas is greater than that of the muscovite variety—about 0·02 for the clear amber and 0·05 for the silver amber variety.

The samples were all about 8 cm. square and 0·1 mm. thick, and the measurements were all made on the Carey Foster bridge at a frequency of 800 cycles per second.

I. Introductory.

When the present investigation was first started over two years ago, it was generally recognised that our knowledge of the dielectric constant of mica was very unsatisfactory. The values given by different investigators and collected in the various tables of physical constants show great variability. Values as widely different as 4 and 8 will be found, and generally speaking there is no obvious reason for such variability. Many condenser makers seemed to think that such large variations of dielectric constant did actually occur, and the differences were not due to experimental errors. It must of course be recognised that "mica" is not a definite chemical compound—i.e., there are many kinds of mica differing in chemical composition, colour and general optical and mechanical properties. These are obtained from various localities, including India, East Africa, Brazil, etc.; but there seemed to be no reliable information as to whether the differences in dielectric constant were greater between different samples from the same source, or between samples of the same kind from different sources. In view of this state of affairs, the British Electrical and Allied Industries Research Association included in its comprehensive programme of research on dielectrics an investigation into the dielectric properties of as wide a range of mica samples as could be obtained. The present Paper is an account of this research, which was carried out for the Association at the National Physical Laboratory, and the authors are indebted to the Association for permission to publish the results.

II. Methods of Measurement.

Probably the most common way of measuring the dielectric constant of material in the form of thin sheets is to form a condenser by pressing two sheets of tin foil into contact with the sample, one on either side of it, and then to measure its capacity. This method has been used by several observers,* but it is well known that the great difficulty attending its use is that of securing intimate contact between the material and the electrodes. On this account electrodes of mercury instead of tin foil have frequently been used; but even in this case, unless special precautions are taken, air bubbles are apt to form on the surface of the dielectric, and so vitiate the results.† Another method consists of making a parallel plate air condenser of known dimensions, measuring its capacity, then inserting a sheet of the material into the gap between the plates of the condenser, and measuring the change of capacity produced. This method has been used by A. Campbell.‡ It of course entirely avoids the above-mentioned difficulty. In the present investigation both the air gap and the mercury electrode methods are used.

(A) *The Air Gap Method.*

(*a*) *The Apparatus.*—The samples of mica were supplied by Messrs. F. Wiggins and Sons. They were all about 8 cm. × 8 cm. square and 0·1 mm. thick. Hitherto the air gap method does not appear to have been carried to the refinement necessary for its use in tests on such samples. The development at the Laboratory of means of producing surfaces which are flat and parallel to a very high degree of accuracy led us to attempt the construction of a standard air gap for these tests, the width of the gap to be from about 0·1 to 0·25 mm. In consultation with the Metrology Department, the form shown in Fig. 1 was arrived at.

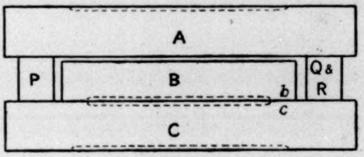

Fig. 1.

The gap consists of three case-hardened and accurately-ground plane parallel steel plates. The upper plate A forms one of the electrodes, and its under surface is plane all over. The top plane surface is an annulus, and is necessary only for the production of the under surface. This plate A is supported by three clear natural quartz pillars, P, Q and R, made to an exact dimension, and each as nearly as possible of the same length. They have plane parallel faces. They were supplied ground to size and polished by Messrs. Adam Hilger. The base plate C has annular plane surfaces above and below. The other electrode consists of the smaller circular plate B, having a complete upper plane surface and an annular lower plane surface, the two being accurately parallel. The thickness of this plate determines the width of the air gap. By the use of plates B of various thicknesses a series of gaps of different widths is obtained. To secure sufficient stiffness of the plates to avoid the possibility of appreciable bending, they were made quite thick—viz., 2 cm. for plate *B* and 2·5 cm. for plates *A* and *C*. These two plates were each 15·5 cm. in diameter.

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* *See*, for example, A. Campbell, Proc. Roy. Soc., A, Vol. 78, p. 196 (1906). Fleming and Dyke, Journal I.E.E., Vol. 49, p. 323 (1912).

† *See*, for example, Bairsto, Proc. Phys. Soc., Vol. 25, p. 301 (1913). R. Appleyard, Proc. Phys. Soc., Vol. 19, p. 724 (1905). J. R. Weeks, Phys. Rev., April (1922).

‡ A. Campbell, Proc. Roy. Soc., A, Vol. 78, p. 196 (1906).

The lengths of the quartz pillars were measured by the Metrology Department and were found to be as follows :—

	Max. length at 62°F.
No. 1	$1{\cdot}9874_8$ cm.
2	$1{\cdot}9874_6$,,
3	$1{\cdot}9874_4$,,
Mean	$1{\cdot}9874_6$ cm.

The faces were all slightly concave, but the amount was quite negligible.

In the present series of measurements three different gaps were used. Their dimensions are given in Table I. The diameter of the gap is, of course, the diameter of the upper surface of plate *B*. In Gap No. 3 this was smaller than in the other two cases.

TABLE I.—*Constants of the Air Gaps.*

Mean height of quartz pillars $=1{\cdot}9874_6$ cm.

Gap No.	Thickness of plate *B*. cm.	Width of gap (*d*). mm.	Diameter of gap. cm.	Calculated capacity *C*. $\mu\mu F$.	Observed capacity C_1. $\mu\mu F$.	Edge capacity C_1-C.
1	$1{\cdot}9747_3$	$0{\cdot}127_3$	11·48	719	732	13
2	$1{\cdot}9625_1$	$0{\cdot}249_5$	11·48	367	382	15
3	$1{\cdot}9747_8$	$0{\cdot}126_8$	7·56	313	333	20

(*b*) *Manipulation of the Gap.*—In order that it may be possible to calculate the width of the gap from the thicknesses of the quartz pillars and the plate *B*, it is, of course, essential that there should be very good contact between the quartz pillars and plates *A* and *C*, and also between plates *B* and *C*. It was found to be very easy to get good contact between the quartz and steel faces, and observation of the interference fringes soon showed when this was attained. It was merely necessary to put the pillar on the steel surface and work it about until the coloured interference fringes disappeared. Good contact between the two steel surfaces *b* and *c* of plates *B* and *C* was not so easy to obtain. If the surfaces were quite dry, there was always an air film between them. To get good contact the surface *C* must be covered with a thin layer of paraffin oil. Plate *B* is then placed in position and worked about. The oil is gradually pushed out from between the contact faces and the contact becomes more and more perfect. When the paraffin layer is so thin as to appear black, the contact is sufficiently good. When all the contacts are good, there is a very considerable force acting across the contact faces and holding the components together, so that the whole apparatus may be lifted by the top plate *A*.

The capacity of the apparatus may be written

$$C_1=C_g+C_e$$
$$=\frac{A}{3{\cdot}6\pi d}+C_e \text{ micro-microfarads,}$$

where the first term represents the actual capacity across the air gap and, the second

C_e represents the " edge " capacity. The actual values for the three gaps used are given in Table I.

The edge capacity of No. 3 is larger than the others, because in this case the sides of plate B are sloping, and plate A overhangs the gap by a greater amount.

The accuracy of reproduction which could be obtained by setting up the apparatus on different days may be judged from the values given in Table II obtained for the capacity of Gap No. 2 on different occasions.

Table II.

Date.	Observed capacity of Gap No. 2.
1st February	382_4 $\mu\mu F$
2nd "	381_7
3rd "	381_5
4th "	381_7
14th "	381_5
30th March	381_4

The imperfection of contact between plates B and C always manifests itself by producing a larger capacity, owing to reduction in the length of the gap.

When not in use the highly polished surfaces were preserved by being kept under paraffin.

(*c*) *Method of Test.*—The gap having been set up as described above, its capacity (C_1) was measured. One of the mica samples was then placed in the air gap, and the capacity (C_2) again determined. From these two observations the dielectric constant of the mica sample is deduced. These capacities may each be considered as made up of two portions. Expressing the capacities in micro-microfarads, we have

$$C_1 = \frac{A}{3{\cdot}6\pi d} + C_3 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

where A = area of mica in the air gap in cm.².

d = width of air gap in cm.

C_3 represents that part of the air gap outside the area A occupied by the mica, and, of course, includes the " edge capacity." Also neglecting edge effects at the boundary of the mica

$$C_2 = \frac{A}{3{\cdot}6\pi\left[d - \frac{t(K-1)}{K}\right]} + C_3 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

where t = the thickness of the mica sample,
and K = dielectric constant of the sample.

Eliminating C_3 from (1) and (2), we obtain

$$K = \frac{C_0 + (C_2 - C_1)}{C_0 - \frac{d-t}{t}(C_2 - C_1)} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3)$$

where $$C_0 = \frac{A}{3{\cdot}6\pi d}$$

C_0 represents the capacity of the area A of the gap, which the mica would occupy, before the mica is inserted.

In the case of Gaps Nos. 1 and 2, the samples of mica lay entirely within the gap, and thus A was the actual area of the mica sample. Gap No. 3 was designed so that when the mica was in position, it projected out of the gap on all sides. In this case A is the area of the upper surface of the middle plate. This third gap was made with a view to the determination of the power factors of the samples, as it was found that when the mica only partly filled the gap, dust particles bridged across the gap outside the mica and vitiated the results. By allowing the mica to occupy the whole area of the gap, this trouble was removed and the leakage avoided.

In order to obtain an idea as to the accuracy obtainable by this method, three samples of best clear ruby mica were tested in each of the three gaps. The results are given in Table III.

TABLE III.

Sample No.	Dielectric Constant.			Mean.
	Gap No. 1.	Gap No. 2.	Gap No. 3.	
1	6·4	7·0	6·6	6·7
2	6·3	6·6	6·5	6·5
3	6·3	6·8	6·5	6·5

(*d*) *Sources of Error.*—It is to be noted that four quantities are required in order to determine K, viz., A (C_2-C_1), d and t, and errors in the determination of these affect the value of K by varying amounts. By differentiation of (3) with respect to each of these quantities we can find the percentage error in K resulting from a 1 per cent. error in A, C_2-C_1, d and t. The results are found to depend on the thickness of the specimen tested. In the case of a typical sample 0·094 mm. thick, the results are as follows (Table IV) :—

TABLE IV.

Gap No.	Percentage error in K due to a + 1 per cent. error in			
	A	C_2-C_1	t	d
1	—2	+2	—5·5	+7·5
2	—4	+4	—5·5	+9·5
3	—2	+2	—5·5	+7·5

Thus it is a disadvantage of the method that errors in the determination of d and t cause comparatively large errors in the value of K. In order to determine t as accurately as possible, a special optical lever arrangement described below was used.

(*e*) *Determination of Thickness.*—It was at first anticipated that the thickness of the mica sheets would be uniform to a considerable degree of accuracy, in the case of the clear mica at least, but on examination of the samples between the flat faces of two quartz pillars, the interference fringes showed that there were local variations of thickness over a circle as small as 1 cm. in diameter. Thus it was necessary to use a measuring device which would give the thickness at a point on the sheet, or at least the average over a very small area, and not merely the maximum thickness over an area as large as 1 cm. A measuring machine having ball contact faces was tried, but the pressure was found to be too great, and an optical lever was designed in which the pressure was quite small. It is shown in Fig. 2. The lever is a bar of steel $13 \times 1{\cdot}8 \times 0{\cdot}5$ cm., having polished steel balls *AA*, 6 mm. diameter, let into it at each end, and a similar ball *B* in the middle, the centre of *B* being displaced about 1 mm. from the line of centres of *AA*. Three gauges, *a*, *a*, *b*, of equal height, are wrung down on to a flat steel plate, and arranged so that the balls *AAB* can rest on their upper surfaces as in Fig. 2. The lever can then turn about the line joining the points of contact of *AA*, and does so until the ball *B* comes into contact with the surface of the gauge underneath it. If, then, a mica sample is placed on the middle supporting gauge, and the lever allowed to come into contact, it is tilted from its zero position, and the angle of tilt is a measure of the thickness of the sample. The angle was measured by the usual lamp and scale arrangement, and the scale was calibrated by using standard gauges of known value for the middle support. On the scale a deflection of 40 cms. corresponded to a thickness of about 0·1 mm. The readings were reliable to less than 0·5 mm. on the scale, so that readings could be made to 0·0001 mm. The middle supporting gauge could not be spherical owing to the displacement of the point of contact when the lever tilts by the rolling of the balls *AA*. The most satisfactory form for this support was found to be a gauge ending in a pin 1 mm. diameter having a flat top (*see* Fig. 2). The lever was provided with an arrestment somewhat resembling that of an ordinary chemical balance. This allowed it to be raised for the placing of the mica in position and then lowered into the same position on the supporting gauges. The scale being made direct reading, it was possible to take a great number of observations very quickly. Nine measurements were made on each sheet at points uniformly distributed over its surface, and the mean taken. A few samples were found to be of practically uniform thickness, but generally there were variations. Sometimes a definite line of discontinuity of thickness was found on a sample of clear mica, the portions on either side of the line being fairly uniform, but of different thicknesses. In other cases the thickness was very variable all over the sheet (this was especially noticeable in the case of mica with inclusions), so that it was impossible to get a value with any accuracy. A 10 per cent. variation of thickness would occasionally be found on one sheet. These results clearly indicate the danger of the formation of air pockets when condensers are built up with tin foil electrodes.

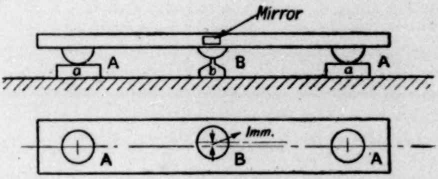

FIG. 2.

(*f*) *The Probable Errors.*—The value of the mean thickness was probably determined to an accuracy of 0·5 per cent. for the better samples. This corresponds to an error in *K* of rather less than 3 per cent. for all the gaps.

The value of *d* was obtained from measurements of the thickness of the quartz pillars and the middle plate. An idea of the accuracy obtained is given by the reproducibility of the gaps. It was probably about 0·25 per cent. for gaps 1 and 3, and 0·12 per cent. for gap 2. This corresponds to errors in K of 2 per cent. for gaps 1 and 3 and 1 per cent. for gap 2.

The errors in A and (C_2-C_1) are of comparatively small importance in gaps 1 and 3, and together will probably cause an error of less than 1 per cent. in K. In the case of gap 2, C_2-C_1 is considerably smaller, and an error of 0·5 per cent. in its measurement is not unlikely. This will cause an error of 2 per cent. in K.

Thus the maximum estimated errors which could occur in the value of K may total 6 per cent. for gaps 1 and 3, and 7 per cent. for gap 2. Typical results on three samples have been given in Table III. The maximum deviation from the mean value is seen to amount to 6 per cent. in the case of gap 2 and 4·5 per cent. in the case of gaps 1 and 3.

(B) *The Mercury Electrode Method.*

(*a*) *Apparatus.*—As previously mentioned, the chief difficulty encountered in using mercury electrodes is the formation of air bubbles on the mica surface. Several forms of electrode were tried with a view to eliminating this trouble. They are shown in Fig. 3.

The first form (*a*) consists of two recessed ebonite plates bolted together and provided with rubber rings, B, to form a mercury tight joint with the mica, which was clamped between the plates. The mercury was poured in through holes in the top of the ebonite plates. On shaking up this arrangement it was noticed that the capacity sometimes increased in value. This indicated that air bubbles were formed. In order to study the mercury-mica contact, one of the ebonite plates was removed, and a mica sheet was pressed up against the rubber ring of the other plate by means of a sheet of glass. On pouring in the mercury, the formation of air bubbles could be seen, and it was found to be due mainly to splashing as the mercury entered. Of the various arrangements tried in order to avoid this, the most successful is shown at Fig. 3 (*b*). The mercury is admitted through fine bore glass tubing entering the ebonite plate from below. A hole in the top of the ebonite plate allows the air to escape. The best size of glass tubes was found by trial (about 1 mm. bore). When this was used, the mercury was seen to enter slowly, and to gradually flow up the mica surface, forming no bubbles as long as the surface was free from dust. On fitting the same sample a number of times into this arrangement, the capacity readings could be repeated with a maximum discrepancy of about 1 per cent., which might easily be due to the yielding of the rubber rings.

A pair of amalgamated copper blocks, Fig. 3 (*c*), was also used. These were 4·5 cms. in diameter and 1·25 cms. thick. Their surfaces were carefully made flat. They were then amalgamated and flooded with mercury. By sliding the mica on to the lower copper block A very carefully it was found possible to get rid of bubbles (when any did form, they could of course be seen through the mica). The top block B presented more difficulty. It was pressed down on to the mica surface, and the excess of mercury removed with a camel hair brush. The blocks so assembled clung together with considerable force, and it was possible to invert the arrangement, remove the block A, and examine the contact surface of the block B. Usually there were air bubbles, and it is unlikely that results could be obtained reliable to less than about 3 per cent.

Experiments were also made with the upper block hollow, and with a narrow rim 1 mm. wide, resting on the mica. The inside of the block was filled up with mercury. (*See* Fig. 3 (*d*)). Here, again, the air bubble difficulty could not be entirely overcome. The mica samples were all tested in the ebonite clamps, Fig. 3 (*b*), which is considered the most satisfactory arrangement.

(*b*) *Errors.*—If C represents the capacity in micro-microfarads of the condenser formed by the mica sample, then, neglecting the edge capacity effect, we have for the dielectric constant

$$K=\frac{3{\cdot}6\pi Ct}{A}$$

where t is the thickness of the sample, and A is the area of either electrode. We notice immediately that this method possesses the advantages (1) that the calculation is much simpler, (2) that errors in C, t and A cause percentage errors in K of only the same amount, whereas by the air gap method all these errors were magnified. The advantages possessed by the air-gap method are (1) there is no difficulty about air bubbles, (2) the edge capacity effect is almost entirely eliminated.

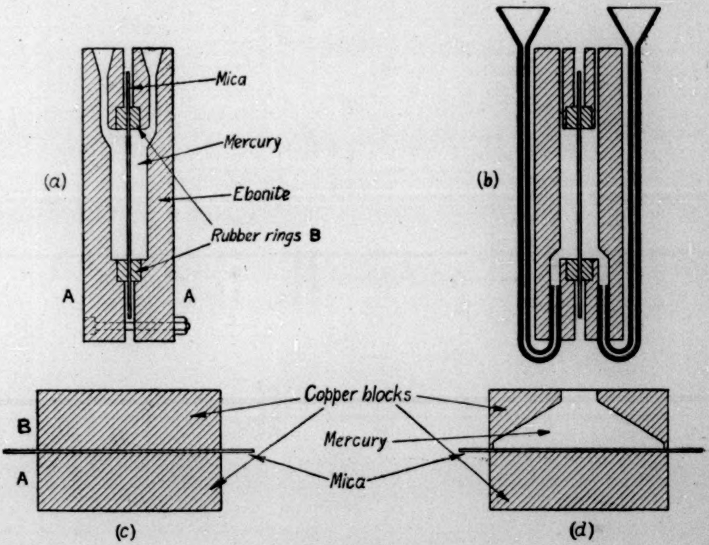

Fig. 3.

The diameter of the mercury electrodes used (Fig. 3 (*b*)) was 5·19 cm. and their thickness 5 mm. The capacity of the condenser formed when a mica sample was inserted was about 1,400 $\mu\mu F$. An estimation of the edge capacity effect was made, using Kirchoff's formula, but it was considered to be not more than $5\mu\mu F$, which is negligibly small for these experiments. The area A was slightly indefinite owing to the yielding of the rubber rings under the clamping pressure, but the error was probably not more than about 1 per cent. Thus the total error to be expected in K found by this method is about 2 per cent., as against 5 or 6 per cent. by the air-gap method.

It must be emphasized that the chief feature of this investigation is the great number of samples examined. More accurate results could easily be obtained on any one kind of mica by carefully choosing a piece of uniform thickness and of a size that could be accurately measured. The uncertainty in the area A of the mercury electrode owing to the yielding of the rubber can be avoided in the case of such a sample by doing away with the rubber and holding the mica sheet between two narrow unyielding rims; but when the thickness of the sample is not uniform the rubber rings are necessary to make the arrangement mercury tight. Over 170 samples were tested by us, and, as previously mentioned, many of them were not of uniform thickness. In testing such a large number of samples the air-gap method has the advantage that, having set up the gap, the mica sheets can be slipped in one after the other and the necessary observations made very quickly. This is one compensation for the somewhat large errors which the method involves. In view of the fact that various observers have obtained such widely different values for the dielectric constant of mica, considerable importance is attached to the fact that most of these samples were tested by both methods, and that the agreement obtained is not entirely unsatisfactory.

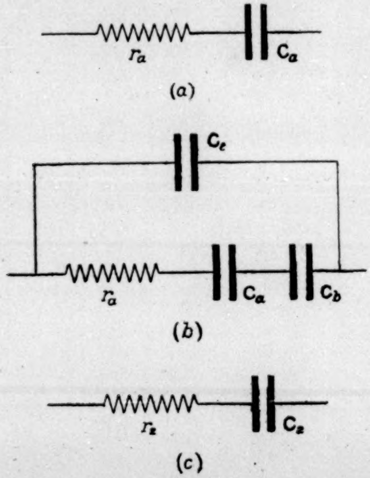

Fig. 4.

(C) *Effect of Conduction and Power Loss.*

Equation (3), giving K in terms of the readings obtained with the air gap, is obtained on the assumption that the dielectric sample is flat, of unform thickness and composition, and everywhere at right angles to the lines of force crossing the gap—i.e., parallel to the sides of the gap. No sample will strictly fulfil these requirements, but in the case of micas of good quality this is not likely to cause serious errors, provided that the true mean thickness of the sample is used. The general consistency of the results on the clear micas is sufficient indication of this.

It is further to be noticed that the derivation of (3) assumes that the potential gradient in the dielectric sample depends only on its dielectric constant—i.e., no account is taken of conduction or absorption in the dielectric. The effect of both these properties is to cause a dissipation of power in the sample under the applied alternating voltage. All the tests were made on the Carey Foster bridge at a frequency of 800 cycles per second. Thus what is actually measured in any one case is the combination of perfect condenser C_a and series resistance r_a (Fig. 4), which is equivalent to the test condenser, under the conditions of test. If r_a, C_a is the equivalent of the mica sheet alone, then, when it is inserted into the gap, the arrangement is equivalent to Fig. 4 (*b*). Here C_b represents the effect of the air space between the mica sheet and the steel plates, and C_e represents the edge capacity. The bridge actually measures the effective resistance r_2 and capacity C_2 of this arrangement (Fig. 4 (*c*)). It is easy to show that for the equivalent arrangements Fig. 4 (*b*) and (*c*) we must have

$$r_2 = \frac{r_a \cdot \dfrac{1}{\omega^2 C_e^2}}{r_a^2 + \dfrac{1}{\omega^2}\left(\dfrac{1}{C_a}+\dfrac{1}{C_b}+\dfrac{1}{C_e}\right)^2} \quad \ldots\ldots\ldots\ldots \quad (4)$$

and

$$\frac{1}{C_2} = \frac{\dfrac{1}{C_e}\left[r_a^2 + \dfrac{1}{\omega^2}\left(\dfrac{1}{C_a}+\dfrac{1}{C_b}\right)\left(\dfrac{1}{C_a}+\dfrac{1}{C_b}+\dfrac{1}{C_e}\right)\right]}{r_a^2 + \dfrac{1}{\omega^2}\left(\dfrac{1}{C_a}+\dfrac{1}{C_b}+\dfrac{1}{C_e}\right)^2} \quad \ldots\ldots \quad (5)$$

In practice r_a^2 is very small compared with

$$\frac{1}{\omega^2}\left(\frac{1}{C_a}+\frac{1}{C_b}\right)\left(\frac{1}{C_a}+\frac{1}{C_b}+\frac{1}{C_e}\right)$$

and thus we have approximately

$$r_2 = r_a \cdot \frac{\left(\dfrac{1}{C_e}\right)^2}{\left(\dfrac{1}{C_a}+\dfrac{1}{C_b}+\dfrac{1}{C_e}\right)^2} \quad \ldots\ldots\ldots\ldots \quad (6)$$

$$\frac{1}{C_2} = \frac{\dfrac{1}{C_e}\left(\dfrac{1}{C_a}+\dfrac{1}{C_b}\right)}{\dfrac{1}{C_a}+\dfrac{1}{C_b}+\dfrac{1}{C_e}} \quad \ldots\ldots\ldots\ldots \quad (7)$$

giving

$$r_a = r_2\left(\frac{C_2}{C_2 - C_e}\right)^2 \text{ approx.} \quad \ldots\ldots\ldots\ldots \quad (8)$$

Equation (3) can be deduced from (7), and thus holds to the same degree of approximation. By the mercury electrode method C_a and r_a are determined directly. By the air-gap method r_a is obtained from (8), and C_a from (7) or (3), which is equivalent to (7).

(D) *Power Factor Determinations.*

At the same time as the capacity measurements were made the values of the effective resistance r_a were also determined, and hence the power factor of the sample deduced in the usual manner from

$$\text{Power factor} = r_a C_a \omega \text{ (approx.).}$$

In the case of the clear micas the power factor was found to be very small, and the reading obtained was very dependent on the condition of the experiment—e.g., when mercury electrodes were used—a higher value of power factor was obtained if the mercury became contaminated with impurities. Different results were also obtained for the same samples on different days, the changes being apparently due to changes in the humidity of the atmosphere. Values as low as 0·0003 were obtained for clear ruby mica, but owing to the difficulty of making sure that the power loss measured was actually dissipated in the mica, the power factor tests on the clear mica were deferred, and it has not been possible to take up the matter again yet. Power factor results for the mica with inclusions were obtained, as owing to the greater power loss in the case of these samples, the above-mentioned difficulties do not arise, and one of our immediate objects was to find out the effect of inclusions on the power factor.

III. RESULTS.

All the samples tested can be divided into two mineralogical groups. They were either

(1) Muscovite (potassium mica) or (2) phlogopite (magnesium mica).

Inside these groups the specimens were divided into various grades. Of these it is convenient to consider two main divisions—

(*a*) Clear micas—i.e., samples which are quite transparent and free from all inclusions, gaseous or solid.

(*b*) Samples with inclusions.

Considering now the clear micas of the muscovite group. They were classed according to colour as ruby, green and brown micas, and, where possible, samples of each kind from various sources were tested. A sample of any one kind usually consisted of eight sheets, and separate tests were made on each sheet. It is unnecessary to give all the results in detail. The results given in Table V for Bengal ruby mica are typical.

It is to be noted that the result by each method is the same for every sheet, within the experimental error of the method used, but the air-gap results are all somewhat lower than those obtained with mercury electrodes. All the samples of the clear micas of the muscovite group gave results very similar to those in Table V. Samples of different colour and from different countries all appear to have practically the same dielectric constant. The values given in Table VI represent in each case the mean for 8 sheets.

TABLE V.—*Dielectric Constant of Bengal Ruby Mica.*

Sheet No.	Dielectric Constant.	
	By Air Gap Method.	With Mercury Electrodes.
1	6·7	7·1
2	6·3	7·1
3	6·5	7·0
4	6·4	7·1
5	6·4	7·0
6	6·6	7·0
7	6·6	7·0
8	6·4	7·1
Mean	6·5	$7{\cdot}0_5$

TABLE VI.—*Dielectric Constant of Clear Mica.*

Mineralogical Group.	Colour.	Source.	Dielectric Constant.	
			Air Gap.	Mercury Electrodes.
Muscovite	Ruby ...	Bengal	6·5	7·0
		Brazil	6·4	7·0
		Madagascar ...	6·4	7·0
		E. Africa	6·5	7·0
	Green ...	Madras	6·6	7·0
		E. Africa	6·7	7·0
	Brown ...	E. Africa	6·5	7·0

In the case of the samples with inclusions, values of both dielectric constant and power factor are given. Where possible, measurements were made both by the mercury electrode and the air-gap methods, and the results of both are given; but in some cases it was impossible to apply the air-gap method. The reason for this is that not only are the samples with inclusions far from uniform in composition, but they are also by no means flat, and when the mica is in the air gap the laminæ of its structure are not equipotential surfaces. Thus conduction occurs along the conducting streaks and patches constituting the inclusions (spots and stains), and the effective capacity of the whole arrangement becomes much larger than would correspond to formula (3). In fact, in many cases C_2 was so large that $(C_2-C_1)(d-t)/t$ was greater than C_0, and the value of K given by (3) was negative. That these effects are due to corrugations in the sample was proved by the fact that samples which gave impossible results in the wide gap gave reasonable results when placed in a narrow gap, which had the effect of flattening out the sheet. On account of these difficulties it would not be safe to trust the air-gap values for the spotted micas without the corroboration of the mercury electrode results. More weight is to be attached to these results in any case, owing to the smaller probable errors.

The results are given in Table VII. As in the case of the clear micas, about eight sheets of each kind were tested, and in the table the mean value is given. In addition, the maximum and minimum values of K and power factor of the sheets of any one kind are given. In the case of a sample of spotted mica, the power factor, and to a less extent the capacity, depends on the part of the sheet tested.

The inclusion of one particular spot in the test may make a large difference. On this account some difference may be expected between the air-gap and mercury electrode results, since the mercury electrodes allowed only the central portion of each sheet to be tested, whereas the sheet being placed in Gap No. 2 an average value for the whole sheet is obtained.

TABLE VII.

Kind of Mica.	Air Gap Results.				Mercury Electrode Results.			
	Dielectric Constant.		Power Factor.		Dielectric Constant.		Power Factor.	
	Mean.	Extreme Values.	Mean.	Extreme Values.	Mean.	Extreme Values.	Mean.	Extreme Values.
Muscovite— Bengal Ruby Clear B (with gaseous inclusions)	6·2	6·0 6·4	0·004	0·001 0·022	7·1	7·1 7·2	0·009	0·005 0·011
Ditto, slightly stained	6·5	6·2 6·8	0·003	0·001 0·005	7·1	7·0 7·2	0·012	0·004 0·036
Ditto, stained	6·4	6·1 6·8	0·016	0·002 0·064	7·1	7·0 7·1	0·006	0·009 0·003
Ditto, much stained	6·5	6·3 6·9	0·026	0·004 0·137	7·0	6·9 7·2	0·005	0·004 0·007
Ditto, spotted	No measurements possible				7·1	6·9 7·3	0·010	0·005 0·017
Madras green, stained	6·7	6·3 6·9	0·054	0·012 0·080	7·1	6·9 7·3	0·010	0·009 0·012
Ditto, spotted	6·7	6·4 7·1	0·002	0·001 0·004	7·0	6·9 7·0	0·001	0·001 0·002
East African, brown spotted	No measurements possible				8·0	6·8 9·8	0·025	0·003 0·070
Madras, brown spotted	6·4	6·0 6·8	0·026	0·001 0·133	7·1	7·0 7·3	0·004	0·002 0·008
Phlogopite— Canadian amber	6·5	5·5 7·5	0·060	0·04 0·11	5·9	5·7 6·1	0·022	0·012 0·052
Canadian silver amber	Measurements worthless				4·8	4·4 5·0	0·055	0·049 0·059

Considering the samples of muscovite, we see that the inclusions have comparatively little effect on the dielectric constant. The value obtained by the air-gap method is approximately 6·5, and that obtained with mercury electrodes is

7·0 or 7·1 for nearly all the samples tested, which include ruby, green and brown micas, stained, spotted, and with gaseous inclusions. These gaseous inclusions were not simply sheets of clear mica in which the laminæ had become detached in places due to mechanical treatment; the inclusions consisted of small circular patches resembling bubbles, and there were usually a large number of them on each sheet. Only in the case of the sheets of East African brown spotted mica was there any considerable variation of K. In this case, values of 6·8 to 9·8 were obtained with mercury electrodes; but even here the samples of low power factor had a dielectric constant of about 7·0. The samples with high values of K were those of high power factor.

The samples of phlogopite—the amber micas—stand out from the others. Sheets of two kinds were tested:—

(*a*) Normal phlogopite, or ordinary amber mica; and
(*b*) The calcined variety, or silver amber mica.

The power losses in both these kinds are considerably greater than in the case of the muscovites, especially in the case of the silver amber samples.

The dielectric constant is lower than that for the muscovite samples, about 5·0 for silver amber mica, and 6·0 for ordinary amber mica, as against 7·0 for all samples of muscovite.

The differences in the values of the dielectric constant given by the two methods have not yet been satisfactorily explained. We have no reason to doubt that the effective values under the conditions imposed by the two methods are as given in the tables, and the differences are greater than the probable errors would lead us to expect. It is possible that there may be a contact effect at the mercury-mica surface. On the other hand, there may be a constant error in the air gap owing to concavity of the surfaces or to the small gaps at the contact of the quartz pillars and the plates. A few tests were made on samples of varnished cloth in the same apparatus. In this case the differences in the values of K obtained were far larger than for mica. Owing to the pressure of other work, we have not been able to continue this investigation, but we hope to resume it at some future date. It may be that there is an electrode capacity effect somewhat similar to that occurring in electrolytes.

In conclusion, we wish to acknowledge our indebtedness to the firm of Messrs. F. Wiggins & Sons, who supplied the mica samples, together with information as to the kinds of mica, and the localities from which they were obtained.

DISCUSSION.

Mr. B. P. **Dudding** asked for fuller information as to the forms, colours and other physical properties of the various types of mica examined.

Mr. P. R. **Coursey**, referring to Table VII. of the Paper, pointed out that the variation in the S.I.C. of different samples of mica of the same type was as great as the variation between samples of different types, and said these variations are perhaps to be accounted for by the fact that the thickness as measured may include the thicknesses of layers of included substances in addition to the mica. This view is confirmed by comparing an optical measurement of the thickness (by means of polarised light) with a mechanical measurement, when a considerable discrepancy is found. A Paper published some years ago in the Physical Review gave a large number of results rather greater than 8·0 for K; possibly the lower values found by the authors may be attributable to inclusions. It is known, for instance, that there are oil inclusions in the mica commonly employed for commutator rings, and, of course, in such cases it is the actual S.I.C. of the impure mica that is of practical importance. The power factor must be attributed to laminæ of imperfectly insulating impurities, and in commercial samples the power factor becomes serious at radio-frequencies, when the apparent S.I.C. may drop to about 3.

Mr. F. E. **Smith** said that the air-gap method appealed to him as a delicate one, but the authors appeared to prefer the mercury-electrode method. He would suggest that the results of the latter might be vitiated by the fact that the actual area of contact of the mercury with the mica is not the same as the area of the exposed portion of the mica, in consequence of the meniscus effect. The cleaner the mercury the more marked would this discrepancy be, and the error in diameter arising from this cause might well amount to 1 mm., which would account for the 6 per cent. difference between the results of the two methods. He would suggest trying the effect of compressing the mercury.

Dr. D. **Owen** said that the Paper represented an accurate and interesting piece of work. Such limitations of accuracy as did occur in the first method were, admittedly, imposed by the extremely small thickness of the specimens ; and this notwithstanding the special care taken in planing and paralleling the metal plates bounding the air-gap. Was there some special virtue in the low value, 0·1 mm., which had been adopted ? Further, in regard to the question of uniformity of the specimens, it would be of interest to know whether the particles of possible foreign inclusions were large enough to be put in evidence by optical tests.

Mr. R. S **Whipple** suggested that some uncertainty arose from the fact that the mica was subjected to the hydrostatic pressure of the mercury, the effect of which would depend on the quantity of air inclusions.

Dr. A. B. **Wood** suggested that a further element of uncertainty was introduced by the fact that the rubber packing would bulge under pressure to an unknown extent, and so affect the exposed area of the mica.

Mr. G. L. **Addenbrooke** said that in his early work he had had some trouble with air bubbles, but had found that, under a pressure of 2 in., mercury will fill up any corner. He personally had used an average pressure of 4 in. or 5 in., and given the container a few sharp knocks to shake the mercury into the corners. Was the mica used by the authors dry ? Any dampness would have a considerable effect on the power factor. Under a microscope mica is distinctly seen to be laminated, and it may contain air films. All such points were, however, of subsidiary interest ; it was particularly interesting to the speaker to see the uniformity of the results obtained by the authors for a great variety of specimens, because he had always suspected that the lack of uniformity in the behaviour of mica condensers was to be attributed to other causes than variation of the S.I.C. ; he had been kept back from pressing this view publicly by the great variation to be found in the values given in tables of physical constants, a variation which was now shown to be erroneous. It was worth mentioning that Post Office engineers assume an all-round value of 6·6 for the purpose of their specifications. He hoped that this valuable Paper, besides clearing up the doubt as to the constancy of the S.I.C. of mica, would call attention to the general necessity for going over the accepted values of such constants again. Existing tables take no account of the power factor.

Mr. A. **Campbell** (communicated) : The Paper removes most of the uncertainty which existed regarding the dielectric constant of mica, and the authors are to be congratulated on the skill and patience with which they have carried out a difficult investigation. One of the main difficulties lies in the very small thickness of the material which has to be tested ; to obtain moderately accurate results the aid of the most refined metrology has to be called in. The writer used the air-gap method in 1906, but the formula he employed was incorrect (due to an algebraical slip). The error was not noticed at the time, as with the particular dimensions used it had very little effect on the results. The correct and complete formula is given by the authors. It is difficult to account for the systematic differences shown by the two methods. They are possibly due to the effect of surface moisture enlarging the effective area in the mercury electrode method. This might be investigated by using solid or liquid paraffin at the joints or by working in a desiccator. The results given for dielectric losses clearly indicate that in making high-class condensers the very best mica must be selected.

Authors' reply (communicated) : We do not think that the discrepancy between the two methods is due to uncertainty in the area of the mica-mercury contact surface, either on account of the meniscus effect or the yielding of the rubber rings. As mentioned in the Paper, the contact surface was examined by replacing one ebonite clamp by a sheet of plate glass, which was pressed up against the mica. In this way the meniscus effect and yielding of the rubber could actually be seen, and their effect on the result estimated. Further, in several cases, the area of the mercury contact was measured by observing the contamination mark left by the mercury on the mica surface. We have no reason to doubt that the value of the area taken was correct within the limits specified. The possibility of surface moisture enlarging the effective area of the electrodes was also considered by us, mainly in connexion with the power-factor measurements. One set of such measurements was made in an enclosure containing dry air, and in another case, using the amalgamated copper blocks, melted paraffin was run over the exposed part of the mica surface. The effect on the apparent power factor was considerable, but on the value of K it was negligible compared with the general experimental errors. We did not include an account of these experiments in our Paper as we were not satisfied with our measurements on the power factor of the best clear mica, and hoped to deal fully with the matter at some future time.

In order to investigate the part played by moisture, some of the samples were tested and then placed (*a*) in dry air for five days, or (*b*) in moisture-saturated air for five days, or (*c*) heated to 35°C. for five days, and then retested. In each case the value of K was the same after this treatment, within the experimental error.

We were aware of the fact that Weeks had obtained values for K which are rather higher than ours, but after a careful consideration of the matter we could not account for this by any inaccuracy in our measurements. We certainly do not think our lower results are due to inclusions. The excellent optical properties of many of our samples were a sufficient guarantee against this. Our value of 7·0 is supported by those of Mattenklodt,* who, it is interesting to note, made his experiments with silver electrodes, i.e., his sheets of mica were silvered. His four tests gave $K = 7{\cdot}1_8$, $7{\cdot}0_6$, $7{\cdot}0_9$, $7{\cdot}6_9$.

The thickness of 0·1 mm. was not definitely chosen by us. Sheets of this thickness happened to be available for the investigation and, in view of the fact that a thickness of this order is used in condensers, it was considered that results on such sheets would be of most value. It must be remembered also that the inclusion of air films is more likely to occur in thick specimens than in thin ones. Any further information required on the colours, forms, &c., of the various types of mica could probably be obtained through the British Electrical and Allied Industries Research Association.

Recent optical tests made for us in the Optical Department of the National Physical Laboratory have shown that the top steel plate is slightly hollow in the central region ; we are of the opinion that this defect, though small, is the cause of the systematic difference between the air-gap method and the mercury-electrode method. The plate will be rectified and a few further measurements should confirm this opinion.

With regard to the effect of the hydrostatic pressure of the mercury on the air inclusions : The mercury was only just above the highest point of the circular area of the electrode and so represented an average pressure of only 4 cms. of mercury. Further, owing to the thinness of the sheets and the good optical appearance of them, it is not thought that air inclusions of any consequence are present.

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* Annalen der Physik, Vol. 27, p. 359 (1908).

V.—THE X-RAY EMISSION OF ELECTRONS FROM METAL FILMS, WITH SPECIAL REFERENCE TO THE REGION OF THE ABSORPTION LIMIT.

By Lewis Simons, *D.Sc.*, *F.Inst.P.*, Reader in Physics in the University of London.

Received September 13, 1924.

Abstract.

From both theoretical and experimental viewpoints it is established that the kinetic energy of emission of electrons from a substance exposed to X-rays may be

$$(\tfrac{1}{2}mv^2)_{K,\,L,\,M,\,\text{etc.}} = h\nu - h\nu_{K,\,L,\,M,\,\text{etc.}}$$

where $h\nu$ is the energy of the incident quantum and $h\nu_{K,\,L,\,M,\,\text{etc.}}$, is the energy associated with one of the " quantum absorption limits " of the substance. As the K quantum limit has greater energy than the L.M., etc., limits, it follows that the K β-rays should have smallest energy. Hence arises the difficulty in giving a quantitative explanation of the selective increase in ionization by collision and absorption when ν exceeds ν_K.

The problem has been approached experimentally by Barkla and Miss Dallas, who have shown how the total energy of the β-rays from silver (as measured by the ionization produced in H_2) varies with wave-length as the region of the K absorption limit is crossed. The author has now determined directly the aggregate number of β-rays of all types emitted over the same range, and shows thereby, using his own previous observations, and also those of the authors quoted, that no selective change in the average energy occurs when the K fluorescent spectrum is excited. It is suggested that this is brought about by a re-grouping amongst the numbers and energies of the β-ray constituents, together with the addition of the K group of minimum individual energy as is required by the above equation.

The experimental work has necessitated the separation of the β-rays from the secondary effects they produce, the chief of which is the production of δ-rays of very low energy. The method of this separation is dealt with and the origin of the δ-rays discussed from the points of view of (*a*) thermionics, (*b*) recoil electrons, recently brought into prominence by Compton and C. T. R. Wilson, (*c*) impact electrons. δ-rays are impact electrons whose most frequent energy is little greater than 3·5 volts, which seems to be independent of the wave-length producing them, and of the substance from which they come. There are indications that they are influenced, at the moment of their departure from the surface, by the scattered X-rays.

The Problem Presented by Selective Absorption and Ionisation.

An attempt has been made to investigate by a new experimental method one of the most intricate problems associated with the absorption of X-rays and the re-emission of the energy in the form of electrons. It is now definitely established that there exists in the mixed stream of electrons emerging from a metal irradiated with X-rays of frequency ν groups whose energies are

$$\tfrac{1}{2}mv^2 = h\nu - h\nu_{K,\,L,\,M,\,\text{etc}} \quad \ldots\ldots\ldots\ldots \quad (1)$$

where h is Planck's constant and $h\nu_{K,\,L,\,M,\,\text{etc.}}$ is the energy associated with one of the quantum limits of the series of emission spectra of the absorbing substance.*

This equation implies that the energy of emission of these photo-electrons may have discrete values ranging from zero, if $h\nu_{K,\,L,\,M,\,\text{etc.}}$ happens to be exactly equal to $h\nu$, up to a maximum of $h\nu$ if the energy of binding of the ejected electrons to the atom from which it is ejected is negligible.

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* For references *see* Simons, Phil. Mag., Vol. 46, p. 473 (1923). Also H. Robinson and F. W. Rawlinson, Phil. Mag., Vol. 28, p. 277 (1914); H. Robinson, Proc. Roy. Soc., A, 104, p. 455 (1923); C. T. R. Wilson, Proc. Roy. Soc., A, 104, pp. 1, 192 (1923).

For a given substance $h\nu_{K, L, M, \text{etc.}}$ may have several different values. It has been established that there are one "*K*," three "*L*," five "*M*," seven "*N*," five "*O*" quantum limits in descending order of energy[*] and corresponding to these there may be "*K*," "*L*," "*M*," etc., groups of photo-electrons in ascending order of energies. In particular the experiments of Sadler,[†] Beatty,[‡] Barkla and Shearer[§] and the author[||] showed that there were always present some electrons possessing practically the whole quantum of incident energy whether or not the *K* spectrum were excited in the absorbing substance; but neither these experiments nor those of De Broglie[¶] and Whiddington[**] gave a measure of the relative numbers of the photo-electrons playing a part in the simultaneous emissions of the *K*, *L*, *M*., etc., types of spectra. That it is a complex phenomenon is apparent from the recent detailed investigation of H. Robinson.[††] M. and L. de Broglie[‡‡] have deduced that 84 per cent. of the energy absorbed is associated with the production of *K* electrons and the remainder with *L*, *M*, *N*., etc., electrons. They also direct attention to the question of whether the stream of *K* electrons itself possesses a greater or smaller amount of energy than that of the *L* electrons expelled by the *same* radiation. The view now generally adopted is that the energy of each *K* quantum and of each *K* electron associated with its emission are, as it were, complementary, the sum of the two energies being equal to that of the incident quantum of energy according to equation (1).

As if in direct opposition to these experimental facts, we have the following: The "*K*" wave spectrum predominates, and as the energy of the individual *K* electron is smallest we might expect on the whole a diminution in the aggregate energy of the corpuscles after the *K* absorption limit has been exceeded in frequency. We might expect, for example, a diminution in the ionization of a gas which is due entirely to corpuscular bombardment,[§§] under these circumstances, yet the ionization relatively to air *increases* in the ratio 3·5 : 1, for example when the Br atom of C_2H_5Br is made to fluoresce with its *K* spectrum and the increase is independent (for a given number of Br atoms) of the nature of the atom absorbing the corpuscular energy.[||||] At the same time there is an increase in the absorption coefficient of the same order. Precisely the same thing is true for the emission of photo-electrons from metals. There is a selective increase in the *total* energy of the corpuscles emitted, accompanied by a selective increase in the absorption coefficient for the incident X-rays. The two alternatives, therefore, are (*a*) when the *K* spectrum is excited the *same* number of atoms as before (when only the *L*, *M*, *N* spectra are excited) is playing a part in the corpuscular emission, and therefore the concomitant of the *K* spectral line must be a rapid corpuscle endowed with a *K* quantum of energy, or (*b*) the selective increase in corpuscular energy under these circumstances is merely a statistical result arising from the now newly existing possibility (i.e., after the *K* absorption limit has been exceeded in frequency) of the breaking down taking place from the *K* level, together with an increased mathematical probability that the break down will occur at this level. This dual process would involve an additional number of orbits for the *K* emission. It could give rise to the observed increase in ionization and absorption, but there would remain over for explanation the mathematical probability alluded to.

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[*] Coster, Phil. Mag., Vol. 43, p. 1070 (1922).

[†] Phil. Mag., Vol. 19, p. 337 (1910).

[‡] Phil. Mag., Vol. 20, p. 320 (1910).

[§] Phil. Mag., Vol. 30, p. 745 (1915).

[||] Phil. Mag., Vol. 41, p. 120 (1921).

[¶] Journal de Physique, VI (ii), p. 265 (1921). C. R., 172, p. 274 (1921).

[**] Phil. Mag., Vol. 43, p. 1116 (1922).

[††] Proc. Roy. Soc., A, 104, p. 455 (1923).

[‡‡] C. R., 173, p. 527 (1921).

[§] C. T. R. Wilson, loc. cit. Simons, Proc. Univ. Durh. Phil. Soc., 5 (i) p. 11 (1913). Jauncey, Phil. Mag., Vol. 48, p. 81 (1924).

[||||] Simons, Proc. Univ. Durh. Phil. Soc., 5 (i), p. 11 (1913).

The difficulty of reconciling the absorption and the ionization phenomena in the region of the absorption band with photo-electric emission phenomena, which undoubtedly are represented by equation (1) is very real. It has been dealt with recently in some detail by Barkla and Miss Dallas,* who state that the *K* ionization is not produced simply, or even principally, by electrons with energy $h(\nu-\nu_K)$. The *K* ionization may be produced by electrons with energy $h\nu$. While there is a possibility that many *K* electrons are emitted with energy $h(\nu-\nu_K)$, no suggested theory which takes them into consideration seems capable of explaining the *K* ionization quantitatively.

The new experiments described below, taken in conjunction with some results previously obtained, and also with those of Barkla and Miss Dallas, throw some light on this complex subject. Whilst the main question remains unanswered experimentally—viz., how are selective ionization and absorption accounted for quantitatively—yet it will be shown that the average energy per photo-electron does not make a selective jump when the *K* absorption limit is passed, and that if the probability question could be settled there would be no need to depart from the well-established Planck-Einstein photo-electric equation.

Experimental Methods.

Shearer† and the writer‡ have shown that when a mixed beam of X-rays is incident upon a metal, two types of electronic discharge occur. One type—viz., the true photo-electrons or β-rays—have already been mentioned ; the other type consists of a copious stream of slowly moving particles or δ-rays, whose most frequent resultant energy corresponds to a fall of potential of only a few volts. Later the writer§ was successful in demonstrating the same effect when the incident X-rays were rendered more or less homogeneous by reflection from a secondary radiator.

The same procedure was adopted in the work described below. For this purpose two extremely thin films were employed, one of gold and the other of silver, laid down electrolytically on paraffin wax.

Table I shows the *K* and *L* absorption limits for silver and gold respectively. These two metals were subjected to X-rays over a range of wave-lengths varying from 388×10^{-11} cm. (K_α rays from Ba) to 871×10^{-11} cm. (K_α rays from Sr). This range of wave-lengths lies well within the region between the *K* and *L* absorption limits for gold, but overlaps the *K* absorption limit for silver. By studying the electronic discharges from silver produced by X-rays whose wave-lengths were round about that of the K absorption limit for silver, and comparing these discharges with those from gold under similar conditions as a control, it was hoped to determine something of the nature of the so-called "K electronic emission." All that can be stated about the thinness of the films is that the graphite upon which they were deposited could be seen through them. The employment of such extremely thin films eliminates as far as possible the necessity for taking into account many factors involved in the absorption of slow electrons in metals about which very little is known at present.*

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* Phil. Mag., Vol. 47, p. 1 (1924).

† Phil. Mag., Vol. 44, p. 793 (1922).

‡ Phil. Mag., Vol. 46, p. 473 (1923).

§ Phil. Mag., Vol. 48, p. 250 (1924).

TABLE I.

——	Absorption limits, cm.	
	K	L
Silver	485×10^{-11}	c. 3500×10^{-11}
Gold	153×10^{-11}	c. 930×10^{-11}

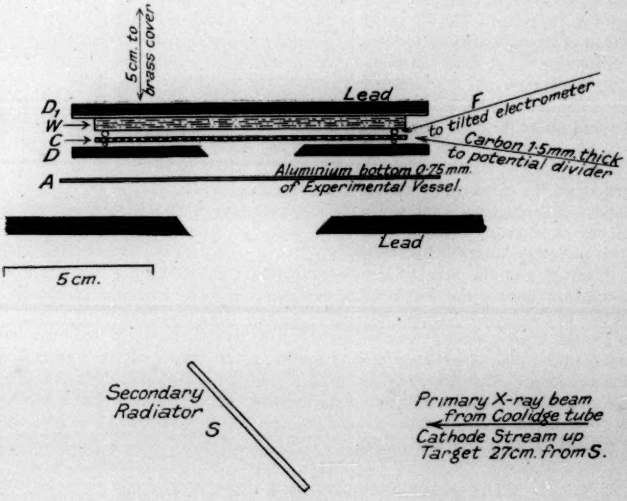
5cm. to brass cover
Lead
D₁
W
C
D
A
F
to tilted electrometer
Carbon 1·5mm. thick
to potential divider
Aluminium bottom 0·75mm. of Experimental Vessel.
Lead
5cm.
Secondary Radiator
S
Primary X-ray beam from Coolidge tube
Cathode Stream up
Target 27cm. from S.

FIG. 1.

The arrangement of the silver or gold film with respect to the incident beam of X-rays is shown in Fig. 1. The primary beam of X-rays was produced by a tungsten target radiator pattern Coolidge tube worked by means of a transformer without rectifying devices. The current through the tube was three milliampères, and the peak potential measured between balls 10 cm. in diameter was 77,400 volts, corresponding to a quantum limit of wave-length of 155×10^{-11} cm., or to a maximum effective wave-length, calculated from Ulrey's constant,* of 358×10^{-11} cm. The characteristic *K* radiation should, therefore, have been excited in all the secondary radiators employed at *S*. This radiation, together with any *L*, *M*, etc., radiation and scattered radiation, passed vertically upwards into the experimental vessel, of which *A* represents the outer aluminium bottom 0·75 mm. thick, then through a hole 3 cm. in diameter in the lead diaphragm *D*, through a thin carbon plate *C* 1·5 mm. thick, and finally fell on to the film *F* of silver or gold on the surface of the paraffin wax plate *W*, 4 mm. thick. The radiation was finally completely absorbed in the brass sheet and lead plate D_1, on top of the wax. The film *F* was connected to a Wilson's tilted electroscope, and the carbon plate *C* to a potential divider, the insulation between them being three small sulphur plugs 2 mm. high. The experimental chamber was exhausted by means of a mercury vapour pump to a mean pressure of 0·035 mm. of mercury, as measured by a Pirani pressure gauge. The factors deciding this pressure were the ease with which it could be established and maintained and the independence of electroscope readings on small fluctuations about this pressure. The electroscope recorded a positive charging of *F*, due to the emission of electrons from the film. The null method of using the instrument when the difference of potential between *F* and *C* was of the order 1 volt or less has been described elsewhere.† Its sensitiveness in order to obtain readable deflections had to be of the order 0·005 volt per division of the eyepiece scale, with a capacity of the system of about 40 cm. No changes whatever were made in the reading arrangements when the gold film was replaced by the silver film at *F*, and, as the X-ray generating devices worked very steadily, a time factor gave the quantity of X-rays falling upon the films. The time of run ranged between two and four minutes. It is presumed throughout that the softer constituents of the charateristic radiations from the secondary radiators have been absorbed by the aluminium and carbon, so that probably none reached the film.

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* Compton and Ross, Phys. Rev., Vol. 13, p. 374 (1919).

Examples of the type of curve obtained for the electronic current from the films plotted against the accelerating or retarding field are given in Fig. 2. The ordinates of these curves represent approximately the number of electrons emerging from the film with a normal energy component (at 180° to the direction of the incident X-ray beam) equal to or algebraically less than the values of the potential given by the abscissæ. The interpretation of that part of the curve on the right-hand side of the ordinate $V=O$ presents many difficulties. It is probable that quite a large proportion of the electrons remain entangled quite close to the surface, and can only be liberated by an accelerating field.

THE SEPARATION OF THE β AND THE δ-RAYS.

Throughout the range of incident wave-lengths employed it was observed that the curves for the electronic emissions from the films began to rise steeply in the region of about —10 volts, the wave-length of the incident rays and the nature of the screen having but little influence on the position of this change. This conforms with Shearer's observation‡ that " the nature of the screen has but little influence upon the velocity distribution of the slow electrons from it." The velocity distribution of these slow electrons seems to be uninfluenced either by the nature of the metal from which they emerge or by the wave-length of the X-rays producing them. In Fig. 2 (*a*) the logarithms of the currents for curves *A* and *B* are plotted against potential. The most frequent resultant energy of these δ or slow rays, taken from the slope of these logarithmic curves, is of the order 3·5 volts.

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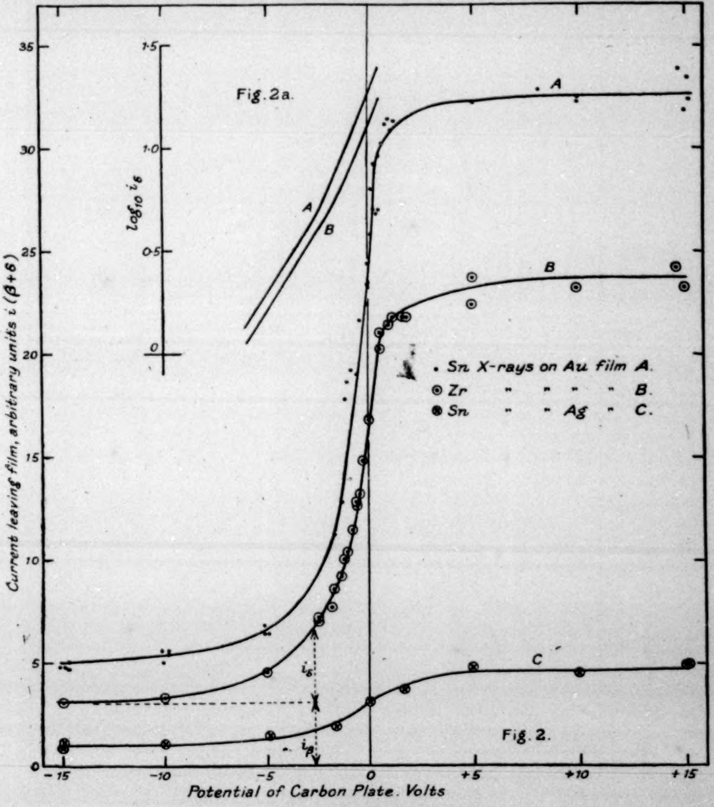
* Phys. Rev., Vol. 11, p. 401 (1918).

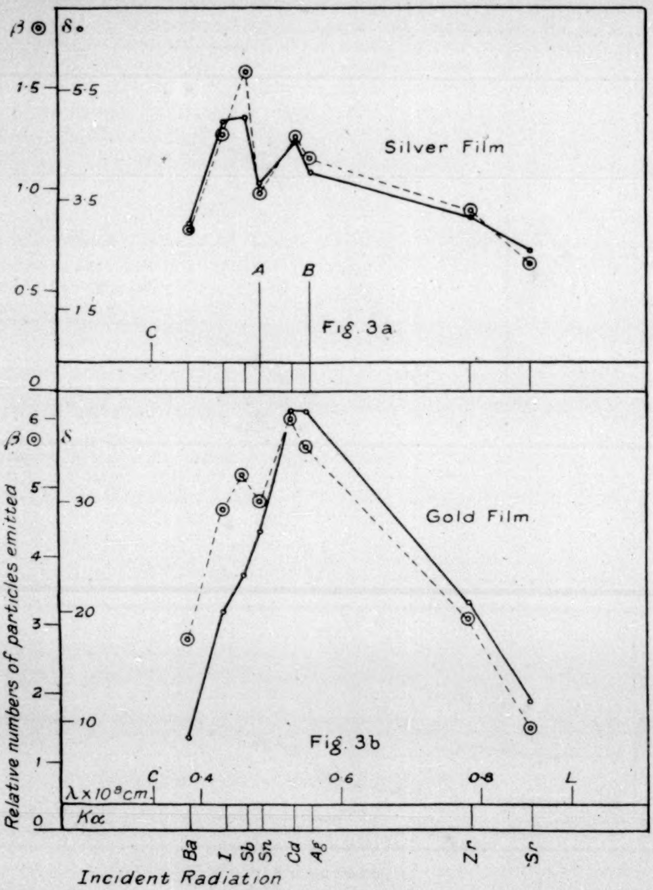
† Simons, Phil. Mag., Vol. 46, p. 473 (1923).

‡ Phil. Mag., Vol. 44, p. 806 (1922).



FIGS. 2 AND 2 (*a*).

There is a slow diminution in the number of electrons emerging from the screen as the retarding potential diminishes below —10 volts. This, together with the general nature of the shape of the curves, mentioned in the preceding paragraph, leads to the conclusion that the region about —15 volts marks off the two distinct



FIGS. 3(*a*) AND 3(*b*).

groups of electrons. The ordinate in this region measures the total ***number*** of β particles escaping from the film, and the difference between the ordinates at —15 and 15 volts, say, measures in a similar manner the total *number* of δ particles which form a second distinct group. These are produced in an entirely different manner from the first.

The relative numbers of β and δ particles emerging from the silver and gold films for the incidence of X-rays of various wave-lengths and under precisely similar conditions are given in Table II. These values are plotted in Fig. 3 (*a*) for the silver film and Fig. 3 (*b*) for the gold film. It is fully realised that the values of the ordinates of these curves involve many unknown factors, such as, for example, the distribution of intensity amongst wave-lengths of the primary radiation from the Coolidge tube, the relative intensity of the secondary radiation from a thick radiator to that of the same constituent contained in the primary, the scattered radiation from the secondary radiator, the influence of density and thickness of the secondary radiators employed, and chiefly the absorption of this incident radiation in the aluminium and carbon of the chamber; but all these are eliminated by the comparison of similarly situated ordinates in Figs. 3 (*a*) and 3 (*b*). This has been done in Fig. 4. In Figs. 3 (*a*) and 4 *AB* represents the breadth of the *K* absorption band, extending from $\lambda=0{\cdot}567\times10^{-8}$ cm. (K_a wave-length for silver) to $\lambda=0{\cdot}485\times10^{-8}$ cm. (*K* absorption limit for silver). *L* represents the approximate position of the *L* absorption limit for gold and *C* the position of the maximum effective output of the Coolidge tube.

TABLE II.

K radiations from	Relative number of Particles emitted when incident upon			
	Silver Screen		Gold Screen.	
	β-parts.	δ-parts.	β-parts.	δ-parts.
Sr $\lambda K_a=871$ X.U. ...	0·63	2·57	1·5	11·9
Zr 788 ...	0·90	3·20	3·0	21·0
Ag 562 ...	1·15	3·99	5·6	38·4
Cd 538 ...	1·26	4·57	6·0	38·5
Sn 487 ...	0·98	3·79	4·8	27·6
Sb 468 ...	1·58	5·00	5·2	23·3
I 437 ...	1·27	4·94	4·7	20·0
Ba 388 ...	0·80	3·05	2·8	8·5

The comparative smoothness of the gold curves, taken under exactly the same conditions as the silver curves above, indicates that the complexity of the latter in the region of the absorption band is real, or in other words, in spite of the necessarily complex nature of each curve, due to causes already indicated, both the shape of each, and the magnitude of its ordinates, are primarily controlled by the metal of the screen—viz., gold or silver respectively.

In Fig. 4 the curve marked β/β shows the ratio of the number of β particles emerging from the silver film to the number emerging from the gold film under precisely the same conditions, whilst the curve marked δ/δ shows in a similar manner the ratio of the δ particles emerging from the two films.

Generally it would appear that the transformation of X-rays into β and δ-rays as measured by their number, not their energy, falls to a minimum within the region of the so-called absorption band, and that there appears to be a special increase in the number of each type of particle on the short wave-length side of the region of the absorption band. The increase is much more marked in the case of the β-particles than in that of the δ-particles. Remembering that it is the numbers of the β and δ-particles that have been dealt with up to the present, it is, nevertheless, surprising that the effects are not more marked in the region of the absorption band than they experimentally appear to be; the selective ionization in a gas beyond the region of the absorption band, the corresponding selective absorption and the emission of line spectra are all such well-marked phenomena.

THE PROPERTIES OF THE β-RAYS IN THE REGION OF THE ABSORPTION BAND.

In the Paper by Barkla and Miss Dallas previously referred to the authors have drawn a curve for

$$\frac{\text{total ionization in } H_2 \text{ by the complete absorption of } \beta\text{-parts . from Ag leaf}^1}{\text{,, \quad ,, \quad ,, \quad ,, \quad ,, \quad ,, \quad Pt \quad ,,}} = \frac{{}_{\mathrm{Ag}}E_\lambda}{{}_{\mathrm{Pt}}E_\lambda}$$

when the β-particles are produced by X-rays of various wave-lengths. The numerical values of these ratios are not given, but this omission does not invalidate the argument that follows. By a suitable adjustment of the ordinate it is at once apparent and somewhat surprising, when it is considered how diverse the two experiments are, that their curve, the broken line in Fig. 4 marked *B* & *D*, shows undoubted similarity to the curve marked β/β obtained in this work. The points on this curve are

$$\frac{\text{number of } \beta\text{-particles emerging from a thin silver film}}{\text{,, \quad ,, \quad ,, \quad ,, \quad gold \quad ,,}} = \frac{{}_{\mathrm{Ag}}n_\lambda}{{}_{\mathrm{Au}}n_\lambda}$$

the β-particles being produced by X-rays over the same range of wave-lengths as those of Barkla and Miss Dallas. Platinum and gold are so near to one another in atomic number, and sufficiently remote from silver, that they may, for purposes of this work, be regarded as equivalent. If we take it for granted that the differences between the dotted curve and the whole line curve at the upper part of Fig. 4 are only due to the great experimental difficulties in measuring the very small electric currents in this work, then we have, on dividing the above ratios by each other,

$$\frac{{}_{\mathrm{Ag}}E_\lambda}{{}_{\mathrm{Ag}}n_\lambda} \Big/ \frac{{}_{\mathrm{Au}}E_\lambda}{{}_{\mathrm{Au}}n_\lambda} = \frac{Af(\lambda)}{Bf(\lambda)}, \text{ where } A \text{ and } B \text{ are constants} = \text{constant.}$$

Omitting now from our considerations the X-ray energy that would escape from the hydrogen in the experiments of Barkla and Miss Dallas during the ionization by impact by the β-particles amongst the atoms, and taking ${}_{\mathrm{Ag}}E_\lambda/{}_{\mathrm{Ag}}n_\lambda = {}_{\mathrm{Ag}}\overline{E}_\lambda$ = the average energy of a β-particle emerging from Ag under the influence of X-rays of wave-length λ, thus it follows that

${}_{\mathrm{Ag}}\overline{E}_\lambda/{}_{\mathrm{Au}}\overline{E}_\lambda$ = constant over the region of the absorption band and through the absorption limit for Ag.

In confirmation of this the author found in some previous work*

$$({}_{\mathrm{Ag}}E/{}_{\mathrm{Au}}E)_{0\cdot562\text{Å. U.}} = 0\cdot43\,;\quad ({}_{\mathrm{Ag}}E/{}_{\mathrm{Au}}E)_{0\cdot388\text{Å. U.}} = 0\cdot48 \text{ (values relative)}$$

when the β-rays were absorbed in air. The wave-lengths quoted are on either side of the K absorption limit for silver, which is at 0·485Å.U. The present work gives

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* Phil. Mag., Vol. 41, p. 120 (1921).

$$({}_{\mathrm{Ag}}n/{}_{\mathrm{Au}}n)_{0\cdot562\text{Å.U.}}=0\cdot21\ ;\quad ({}_{\mathrm{Ag}}n/{}_{\mathrm{Au}}n)_{0\cdot388\text{Å.U.}}=0\cdot28 \text{ (values relative).}$$

Hence

$$({}_{\mathrm{Ag}}\overline{E}/{}_{\mathrm{Au}}\overline{E})_{0\cdot562\text{Å.U.}}=2\cdot0\ ;\quad ({}_{\mathrm{Ag}}\overline{E}/{}_{\mathrm{Au}}\overline{E})_{0\cdot388\text{Å.U.}}=1\cdot7$$

The K, L, M, etc., corpuscles being excited from the silver in the latter case and only L, M, etc., corpuscles in the former case.

The approximate equality of the numbers 2 and 1·7 and the close similarity between the curves in the upper portion of Fig. 4 yielding the results of the last two paragraphs point strongly to the important conclusion that there is no *selective* increase in the average energy of the β-rays emerging from the silver film as the region of the absorption limit of the X-rays incident upon it is passed across. It will be seen that if this conclusion can be generalised, selective increase in ionization and absorption does not involve a similar selective increase in the average individual energy of the electrons taking part in the action ; but it does involve a selective increase in the number of electrons participating. On the excitation of the K fluorescent spectrum there will be added to the resultant electron stream a comparatively large number of K electrons (of smaller individual energy than L, M, etc., electrons), which, ionizing by collision, produce the selective increase in ionization in the case of a gas. Selective absorption is the accompanying phenomenon

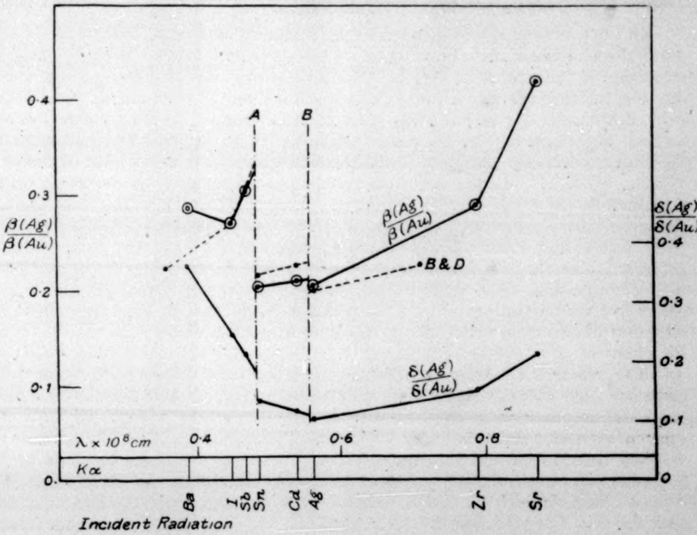

FIG. 4.

of the participation of a large number of additional atoms whose K levels are disturbed. Reference must here be made to two recent successful theoretical contributions, one by Kramers* and the other by Jauncey† on the subject of the change in the absorption coefficient over the region of the absorption limit. The treatment in both cases is statistical, and assumes that the K special absorption is allied to the increased probability of the interaction between the radiation and a new additional set of K orbits. This is the first direct confirmatory experimental evidence of the line of argument adopted by these two writers. The author attempted some years ago to obtain on statistical lines a theoretical expression for this change in the absorption coefficient.‡

THE δ-RAYS.

Of the δ-rays produced throughout the thickness of the film, only those liberated quite close to the surface have a chance of escaping. This is shown by the great steepness of the curves of Fig. 2. Electrons which have a most frequent energy due to a fall through only a few volts cannot have been liberated in the first instance from deep molecular layers. We must therefore regard the δ-rays observed as a surface phenomenon. In his recent work, C. T. R. Wilson§ has calculated the value of the average energy of the δ-electrons liberated in a gas to be of the order 7 volts, whilst it is evident from the rounded shape of the curves of Fig. 2 on the right-hand side of the axis $V=0$ that the value of 3·5 volts for the most frequent energy of the δ-particles from a metal should have a comparable quantity added to it, due to the entrainment of a large number of the very slow particles in the surface of the film. It has already been remarked that neither the nature of the metal from which they come nor the wave-length of the X-rays producing them seems to have any influence upon the velocity distribution. The curves show that very few of the electrons of this group have a resultant energy greater than about 40 volts.

The presence of this group of particles is such an outstanding feature that I have been led to examine in some detail the method of their production. These particles, together with an extremely absorbable radiant energy, accompanying their production, must be regarded as the "end-products" of X-rays. As the velocity distribution curves show such a remarkable similarity to those for the thermions emitted from hot bodies as in Richardson's work, the experimental conditions being similar, the author carried out a numerical investigation,‖ assuming that the distribution in speeds was Maxwellian, and employed Richardson's formula for the normal components of the energy, viz.,

$$\log_e i/i_0 = -\frac{ne}{RT} \cdot V_1,$$

where V_1 is the retarding potential reducing the normal δ current from i_0 to i, $ne=0\cdot43$ e.m.u. and $R=3\cdot7\times10^3$ erg/deg.C.¶ In this manner a temperature T was calculated which, on the hypothesis of the similarity between the two classes of phenomena, could be regarded as that due to atomic recoil of the centres yielding the primary β-particle, these hot centres momentarily becoming thermionic (δ-ray producing). But the temperature calculated from the slope of the log. lines of Fig. 2 (*a*), using the above formula, is altogether too high to be accounted for by atomic recoil consequent upon the ejection of even the most rapid β-particles available. The thermionic process does not offer any clue to the production of these extremely slow electrons.

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* Phil. Mag., Vol. 46, p. 836 (1923).
† Loc. cit.
‡ Trans. Roy. Soc. S. Africa, 6, 4, p. 311 (1917).
§ Loc. cit.
‖ Phil. Mag., Vol. 46, p. 473 (1923).
¶ "The Emission of Electricity from Hot Bodies," p. 144 (1916).

The possibility suggests itself that the δ-electrons are themselves the recoil electrons, consequent upon the general scattering process of the incident X-rays. Though this is untenable for reasons stated below, yet the experimental results strongly suggest some relationship to the recently published quantum theory of scattering by Compton.* As a consequence of this theory, Compton and Hubbard† have deduced that the energy of the recoiling electron will be

$$K=\frac{2ah\nu_0\cos^2\theta}{(1+a)^2-a^2\cos^2\theta}$$

in which $h\nu_0$ is the value of the incident quantum, $a=h\nu_0/mc^2$, m being the mass of an electron and c the velocity of light, θ is the angle between the direction of recoil of the electron and that of the incident quantum, the permissible values of θ ranging between 0° and 90° round the direction of the incident quantum. This expression varies as $\cos^2\theta$ for the small values of a satisfied in the present experiments. Now a thin film exposed normally to the incident beam of X-rays would emit from the faces of incidence and emergence a similar distribution of characteristic X-rays (i.e., characteristic of the film), each quantum of which, before passing away from either face, should stand an equal chance of being scattered. The recoil electrons due to the scattering of the characteristic rays should therefore be equally distributed on both sides of the film. This is not the case, however, when the incident beam is directly scattered. Those quanta which return at an angle of 180° to the direction of the incident quanta, thereby suffering a maximum degradation of energy, will yield a recoil electron of maximum energy in a direction making an angle $\theta=0°$ to that of the incident quantum, whilst those passing on unscattered will yield a recoil electron of zero energy in a direction of $\theta=90°$ round that of the incident quantum.

In the former of these two cases we have

$$K_{\text{max.}}=h\nu_0\,.\,2a/(1+2a),$$

and if we consider that the only recoil electrons we are concerned with in these experiments are those produced by the scattering of the X-rays characteristic of the gold film itself, we have ${}_{\text{Au}}\lambda_L=1{\cdot}273\times10^{-8}$ cm. $\therefore\ a=0{\cdot}019$, making $K_{\text{max.}}=357$ volts. It is scarcely probable, therefore, as the recoil electrons vary in energy from 357 volts down to zero, that practically all of them should lie within the range of energy 0 to 30 volts, with a most frequent energy of 3·5 volts, determined from the curves of Fig. 2 (*a*), and that the velocity distribution should be experimentally independent of the nature of the screen from which they emerge, which also enters into the above calculation. The same thing is true if we adopt the view expressed recently by C. T. R. Wilson,[*] in which the incident quantum is scattered uniformly in all directions. The maximum value of the recoil velocity is now u in the expression $h\nu_0=cmu$, where $h\nu_0$ is the energy of the incident quantum. On this theory, too, the energy of the recoiling electrons would vary from about 96 volts downwards, and be dependent upon the metal of the screen. The experimental evidence is thus against the view that the δ-rays are recoil electrons.

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* **Phys. Rev., Vol. 21, p. 486 (1923).**

† **Phys. Rev., Vol. 23, 4, p. 439 (1924).**

Returning, therefore, to the view that the δ-rays are produced by the impact of the rapid β-rays on the atoms, it remains unexplained why the group should be so sharply marked, or, in other words, why only those electrons most loosely bound to the atoms should be removed by the impact of the energetic β-particle. This is one of the most remarkable features of the cloud tracks of both α and β-particles. Fig. 3 (*a*) shows clearly that there is a simple proportionality between the number of β-particles and the number of δ-particles produced by them as they emerge from a silver film. In the case of the gold film, Fig. 3 (*b*), this proportionality is not so marked. The difference between the two cases receives a tentative explanation from the following experimental observations. Dauvillier[†] states that the Compton effect—i.e., the increasing of the wave-length of X-rays after scattering—only appears in the case where the selective absorption is small, and C. T. R. Wilson[‡] has formulated an hypothesis that characteristic X-rays can be transmitted by a "handing-on" process along the path of a β-particle. As the energy of the β-particles is so great, they would be imperceptibly influenced by the scattered X-rays at the moment of their emergence from the film. On the other hand, the impact electrons, on account of their small energy, may be profoundly influenced at the moment of emergence by the radiations scattered backwards from the face of incidence. The distribution of intensity amongst wave-lengths of this scattered radiation would be the same as that of the incident radiation, but it would suffer a general increase in wave-length given by[§]

$$\triangle\lambda=(2h/mc)\times(\text{mean value of }\sin^2\varphi/2)$$

φ is the angle between the incident and emergent quanta. In these experiments the scattered rays emerge from the face of incidence of the X-rays on the film so that φ ranges over from 180° to 90°,

$$\therefore\triangle\lambda=0{\cdot}04\text{ Å.U.}$$

Figs. 3 (*a*) and 3 (*b*) exhibit all these effects. There is a lateral shift between the δ-ray and β-ray distribution curves from gold of approximately 0·04 Å.U. This effect is not shown in the case of the emissions from silver, because of the absorption band coming in the middle of the curve.

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DISCUSSION.

Dr. D. OWEN expressed admiration of the skilful disposition of apparatus used by the author. Having had the opportunity of actually seeing the apparatus as set up he felt certain that the measurements recorded in the Paper were amply worthy of careful consideration.

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[*] Loc. cit.

[†] Nature, Vol. 114, p. 111 (1924).

[‡] Loc. cit., p. 209.

[§] Compton, Phys. Rev., Vol. 21, p. 486 (1923).

Owing to the lateness of the hour, the **President** closed the discussion with the remark that the Paper constituted a contribution of the highest value to the Proceedings of the Society. He asked that remarks by other intending speakers should be forwarded to the Secretary.

Prof. A. O. **Rankine** (communicated) : As there is no sharp line of demarcation between the β and δ-rays would it not have been better to have taken as the measure of the β-ray emission the ordinate of Fig. 2 corresponding to the minimum ionizing potential of an atom instead of fixing upon the ordinate at the arbitrary point —15 volts ? Again in deducing the main result of the Paper use is made of two quantities, (*a*) the total energy of photo-electric emission as measured by the number of pairs of ions produced in a gas by the complete absorption of photo-electron energy, and (*b*) the number of photo-electrons engaged in ionizing. The former of these two quantities contains all the effects right down to any ionization produced by the slowest particles, whilst the latter excludes the possible ionizing effects of particles whose resultant velocity is less than 45 volts. Is it legitimate, therefore, to draw any conclusions from the ratio of (*a*) to (*b*) ?

Author's reply (communicated) : These very apposite questions have received careful consideration during the writing of the Paper. The ordinate at —15 volts in Fig. 2 measures the total number of β-rays present whose *resultant* velocity is greater than 45 volts. The choice of this point has been considerably influenced by Shearer's curve (Phil. Mag. 44, p. 806 (1922)), which has been carried down to —180 volts, by C. T. R. Wilson's remarks (Proc. Roy. Soc., A. 104, p. 211(1923)), and by the constant similarity between all the curves I have taken. The ordinate I have taken, viz., that at the foot of the *only* steep fall that exists, so far as I know, along the current-potential curve marks the dividing line between the number of primary β-particles and of the secondary effects produced by them. These secondary effects include the production of electrons of all speeds up to 45 volts, which themselves may produce on the average 3 or 4 more electrons each which cannot ionize further. An essential feature of the work has been to sort out the true, primary, high-energy, β-ray emission from all the low-energy secondary collision effects produced by them.

C. T. R. Wilson has shown that the primary ionization is about 90 pairs of ions per cm. in air for β-rays of velocity about 10^{10} cm. per sec., which is a rough estimate of the velocity of the primary β-rays whose numbers have been measured in this Paper. The total ionization per cm. including that due to the secondary β-particles is about 3 or 4 times as large as the primary.

It is in view of these considerations that I have chosen the ordinate I have as measuring the number of primary β-rays, whilst the other references in the Paper to the work of Barkla and Miss Dallas and to my own previous results undoubtedly give the *total* energy of these particles. Perhaps it would have been better had I said that there was no evidence of a selective increase in the average energy of the *primary* β-particle over the region of the absorption limits.

DEMONSTRATION OF AN ELECTRICAL METHOD OF PRODUCING VOWEL SOUNDS AND ITS APPLICATION TO WIRELESS TELEGRAPHY.

By Dr. W. H. Eccles, *F.R.S.*, and C. F. A. Wagstaffe.

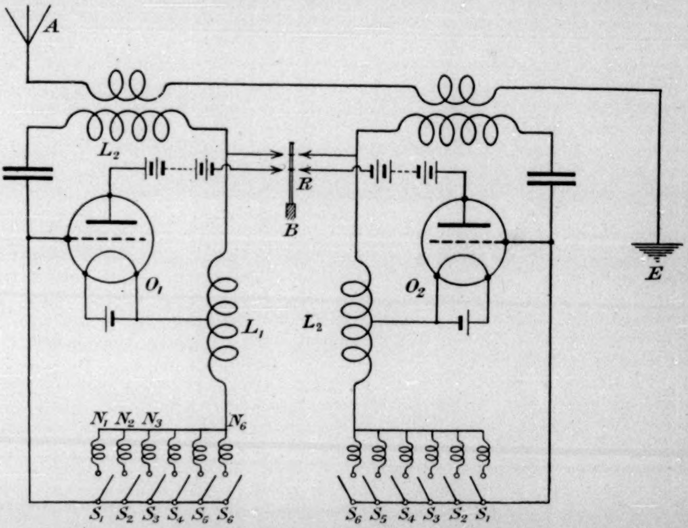
THE apparatus to be shown represents wireless sending and receiving stations. It was devised to test a proposed method of communication in which the electric waves transmitted could produce easily recognizable chords at a suitably equipped receiving station. It was thought that chords would be more easily recognised than simple tones, and that vowel sounds would be the most easily recognised of chords.

The ideal aimed at in this method of communication is that an operator at one side of the ocean should converse with an operator at the other side by means of a language made up of vowel sounds (somewhat like the Hawaiian language), the sounds being produced by pressing keys. Any ordinary language could be converted immediately into this telegraphic form by a code wherein each consonant was represented by a definite pair of vowel sounds.

Consider the problem of transmitting the vowel *ah* from a wireless station utilising a frequency of, say, 100,000 cycles per second. One way is to pronounce the vowel in front of a microphone arranged for wireless telephony. But if the human voice is not to be employed, if, in fact, an artificial vowel sound is to be produced by pressing a key, vibrations of a voice frequency such as 250 cycles per second must be combined, according to the analysis of Sir Richard Paget, with the characteristic frequencies 800 and 1,300. Then these must be impressed upon the oscillations of 100,000 frequency by a microphone or otherwise. The simplest electrical solution is to employ a buzzer of frequency 250 cycles per second to excite at each stroke two circuits tuned respectively to 800 and 1,300 cycles per second, and to pass the complex currents through a telephone to convert them to sound waves. Such an apparatus has been used for testing telephones by the Western Electric Co. without any thought of applying it to wireless signalling. For this latter purpose the sound from the telephone could be applied to a microphone, or, alternatively, the complex currents could be used directly to modulate the high-frequency oscillations.

The method to be described is, however, more direct than that just suggested, and currents of acoustic frequency need not appear at any stage earlier than in the telephones of the receiving operator. Broadly, the method is as follows: At the transmitting station the pressing of a key produces immediately electrical oscillations of frequencies 100,800 and 101,300, and waves of these two frequencies are radiated. At the receiving station the waves produce corresponding oscillations, and these are combined with a locally generated frequency of 100,000 cycles per second in the well-known "heterodyne" manner so as to produce currents of frequencies 800 and 1,300. In order that these should together simulate the vowel sound *ah*, a current of voice frequency, say, 250 cycles per second, should be superimposed in a suitable way; this may be done either before the oscillations leave the sending station or after they reach the receiving station. Each method has given good results with a variety of vowels, made up by taking Paget's table of vowel constituents.

Three distinct ways of carrying out the above conception have been tried successfully. In the first of these ways a pair of oscillating circuits, which may be coupled together in any manner, or which may be completely separated, are adjusted to have the two natural frequencies of, say, 100,800 and 101,300, and are excited by "musical spark plant of, say, 250 cycles per second. The consequent oscillations are passed to the sending antenna. If the two circuits are coupled and excited by one and the same spark, the two frequencies natural to the coupled circuits are partly determined by the coupling. At the receiving stations the oscillations are amplified if necessary and heterodyned by aid of a local oscillator of frequency 100,000 per second. The resulting compound current is rectified in any manner and passed to the operator's telephones. The tone heard there possesses the quality of the vowel *ah*. Other vowels can be made similarly by associating other high-frequency circuits with the same spark apparatus, and thus intelligible signals can be transmitted by aid of a code. Instead of the heterodyne the apparatus known as the tone wheel and other apparatus may possibly be employed.



A ANTENNA. *E* EARTH. O_1 TRIODE OSCILLATOR FOR UPPER FREQUENCIES.
O_2 TRIODE OSCILLATOR FOR LOWER FREQUENCIES.

The second way of working employs two sources of continuous waves, one of frequency 100,800, say, and the other of frequency 101,300 to produce waves. The sources are modulated or interrupted at equal acoustic frequencies of, say, 250 per second, either by the same or different pieces of apparatus. The waves of frequency 100,800 may be emitted at the same instant of time as those of frequency 101,300, or they may be emitted alternately, as may be advantageous in cases where one and the same interrupting apparatus is applied to both sources of continuous waves. In this latter event a physiological phenomenon which may be called the persistence of audition has been discovered and is relied upon to blend the results at the receiving station. Reception is carried out by heterodyning at 100,000 per second or by an analogous process. The sound heard in the telephones has the quality of the vowel *ah* superposed upon a " voice " of frequency 250 per second. The figure shows diagrammatically apparatus suitable for this latter way of working when six vowel sounds are to be produced and employed. It consists of two similar triode oscillators O_1, O_2, each capable of oscillating at any one of six chosen frequencies. The choice of frequency is given by the switches $S_1, \ldots S_6$. When S_1 is closed, the inductance in the left-hand oscillating circuit is made up of the coils L_1 and L_2 and N_1; when S_2 is closed, N_2 is substituted for N_1; and when S_3 is closed, N_3 is substituted. The respective circuits so formed are conveniently tuned to the desired frequencies by moving the N coils with respect to the L coils whereby the mutual inductance between the respective pairs is varied. Alternatively, small condensers have been used instead of the N coils. A buzzer B carrying a contact rod R is maintained in vibration at 250 per second and alternately stops and starts the oscillators by making and breaking their circuits. The two switches marked S_1 are closed simultaneously, preferably through insulating linkages, when a key (not shown in the figure) is depressed; the switches S_2 are similarly closed by another key, and switches S_3 by a third key. Thus by pressing one of the three keys the operator can transmit in one operation the two components which make up a " vowel " signal. It is found practicable to build and work apparatus of this type employing six vowel sounds familiar in the English language. By manipulation of the keys consonantal effects can be produced. It is not necessary to adhere to the vowel sounds of one language, or, indeed, to the vowel sounds of any existing language, in order to make easily recognisable complex sounds.

The third example of this class of apparatus employs two sources of continuous waves, one of frequency 100,800, say, and the other of frequency 101,300, to produce waves, and these are transmitted from the sending station without modulation. At the receiving station the received oscillations are heterodyned by a local oscillator of frequency 100,000 whose oscillations are modulated by local apparatus at a frequency of, say, 250 per second, and the result rectified and passed to the telephones. The " voice " heard in the telephones is of frequency 250 per second, and the vowel quality, being determined by the heterodyned high-frequency oscillations, is that of the vowel *ah*.

In the apparatus exhibited there is provided an oscillatory circuit of acoustic frequency which has been calibrated so that various settings of the condenser give to the circuit the twelve acoustic frequencies required for the six vowels. This circuit is employed as an electrical resonator for the adjustment of the heterodyne currents accurately to their correct frequencies.

DISCUSSION.

Sir Richard Paget said that the Demonstration proved conclusively that effects of the kind described can actually be obtained. He would suggest experimenting with the simplest form of electrical sound-producers on the lines followed by Mr. J. Q. Stewart, of the Western Electric Company of America, in the testing of telephone receivers. In such apparatus low-frequency resonant circuits are employed and the necessity for heterodyning is avoided; and if, as the speaker believed, consonant sounds consist of not more than four components, it should not be difficult to synthesise ordinary speech in a telephone. Such apparatus would not, of course, be applicable directly to telegraphy, but would be of considerable theoretical interest.

VI.—NUMERICAL VALUES OF CHEMICAL CONSTANTS AND FREQUENCIES OF THE ELEMENTS.

By ALFRED CHARLES EGERTON, Clarendon Laboratory, Oxford.

Received August 7, 1924.

ABSTRACT.

(1) The experimental evidence for the general chemical constant C_0 possessing the theoretical value (—1·589) is collected.

(2) Tables of chemical constants are given and data collected for the determination of characteristic temperatures of the elements.

(3) A linear relation between logarithm of mass and characteristic temperature is indicated.

INTRODUCTION.

IT has been pointed out (Egerton, Phil. Mag., 39, p. 1, 1920) that the value of Stephan's constant can be determined from the value of the chemical constant obtained from vapour pressure measurements, and that the value so obtained in the case of mercury did not agree satisfactorily with that obtained from the later measurements of total radiation. The object of this communication is to modify that statement in accordance with recent work on vapour pressures and to show that there is no longer a general discrepancy. Accepting, then, the latest figures for radiation constants, the values of the chemical constants of the elements are computed. Values of vibration frequency are also calculated for the same elements. The chemical constant depends on the logarithm of atomic weight, and it is noticed that the frequencies of some of the closely related elements can be expressed as a linear function of log M.

NUMERICAL VALUES OF CHEMICAL CONSTANTS.

The chemical constant or the constant of the vapour pressure equation for a solid element with monatomic vapour, viz.,

$$\log p = -\frac{\lambda_0}{RT} + \frac{C^g{}_p}{R}\log T - \int_0^T \frac{dT}{RT^2}\int_0^T C^s{}_p dT + \text{constant}$$

is given by the Sackur relation $C = 3/2 \log M - C_0$ where

$$C_0 = \log \frac{2\pi^{3/2}R^{5/2}}{N^4h^3}$$

(M is the atomic weight, N the Avrogadro number, h Planck's constant). C_0 becomes $-1{\cdot}589$, if $h = 6{\cdot}554 \cdot 10^{-27}$, $N = 6{\cdot}061 \cdot 10^{23}$, $R = 8{\cdot}315 \cdot 10^7$ or ($k = 1{\cdot}372 \cdot 10^{-16}$).

The figure implies, or alternatively can be based upon, values of the Stephan constant $5{\cdot}709 \cdot 10^{-5}$, or Wien's constant 0·2885, which are in close agreement with the most recent experimental values* (c.f. Birge, Phys. Rev., 14 . 361, 1919; and Nature, June 16, p. 811, 1923).

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* NOTE.—These are the values accepted for the compilation of Critical Tables of Constants and are in close accord with those cited in Landolt Bornstein (1923 edition).

Experiment justifies the application of the Sackur relation; the evidence is collected in the following table. It depends entirely on vapour pressure and specific heat measurements, except in the cases of bromine and chlorine :—

TABLE I.

Element.	C Expl.	C_0.	Reference.
Mercury	$1{\cdot}83 \pm 0{\cdot}03$	$1{\cdot}62 \pm 0{\cdot}03$	Nernst
Cadmium	$1{\cdot}45 \pm 0{\cdot}1$	$1{\cdot}63 \pm 0{\cdot}10$	Egerton. Braune.
Zinc	$1{\cdot}10 \pm 0{\cdot}1$	$1{\cdot}62 \pm 0{\cdot}10$	,, ,,
Lead	$1{\cdot}8 \pm 0{\cdot}2$	$1{\cdot}67 \pm 0{\cdot}20$	Egerton
Chlorine	$0{\cdot}80 \pm 0{\cdot}1$	$1{\cdot}52 \pm 0{\cdot}1$	Henglein
Bromine	$1{\cdot}27 \pm 0{\cdot}1$	$1{\cdot}58 \pm 0.1$	,,
Argon	$0{\cdot}79 \pm 0{\cdot}04$	$1{\cdot}61 \pm 0{\cdot}04$	Born
Hydrogen	$-1{\cdot}11 \pm 0{\cdot}03$	$1{\cdot}57 \pm 0{\cdot}03$	Simon
Cæsium	$1{\cdot}64 \pm 0{\cdot}16$	$1{\cdot}54 \pm 0{\cdot}16$	Scott
Rubidium	$1{\cdot}36 \pm 0{\cdot}18$	$1{\cdot}53 \pm 0{\cdot}18$	,,

The weighted mean would become $1{\cdot}596 \pm 0{\cdot}008$, a value very near that deduced above from the accepted basic constants. The small discrepancy pointed out previously (Phil. Mag., 39, p. 1, 1920) seems to exist no longer.*

NUMERICAL VALUE OF FREQUENCIES.

The tables which follow include the chemical constants for the monatomic vapour of the elements based on the value $C_0 = -1{\cdot}589$. These tables also include the characteristic temperatures ($\beta\nu$) computed according to the Lindemann melting point formula (Column 7) and according to a formula based on the coefficient of expansion (Column 8) from the data given in 3, 4 and 5, collected from Landolt Bornstein (1923 edition) and other sources. Doubtful values are placed in brackets. $\beta = h/k = 4{\cdot}778.10^{-11}$.

Previous computations of the values of $\beta\nu$ have been made by Lindemann and subsequently by Biltz (Zeit. Elektrochem, 17 676, 1911) on the basis of Lindemann's formula, $\nu = K\sqrt{\frac{T_s}{MV^{2/3}}}$. The constant K also used by Biltz had the value $2{\cdot}12 \times 10^{12}$, instead of $3{\cdot}1 \times 10^{12}$, used above. Blum (Ann. d. Phys., (4) 42, 1,397, 1913) has compared the merits of the formulæ of Lindemann, Einstein, Debye, Gruneisen and others. Since $T_s a$ is approximately constant (as pointed out by Lindemann), the formula $\nu = 4{\cdot}2{\cdot}10^{11}\sqrt{\frac{1}{Mav^{2/3}}}$ also applies, T_s is the melting point and a the coefficient of expansion; Blum showed that it agreed well with experimental results. This formula is related also to that of Grüneisen when $C_v = 3R$. None of the formulæ take into account the anisotropic nature of the solid elements. On the whole, the Lindemann melting point formula agrees closest with experimental results. The figures in brackets in the last column have been calculated from the available specific heat data, but only in some cases are the data reliable. The values not in brackets are generally accepted as correct. Although the measurements may be satisfactory, the state of the metal may cause considerable difference in the specific heats, e.g., for calcium two forms have been noted by Eastman, Williams and Young (J.A.C.S., 46, 1178, 1924), giving differences in specific heat, likewise in the case of tin (*see* Brönsted, Zeit. Phys. Chem., 88, 479, 1924). The values for iron also differ widely. In some cases (e.g., sulphur) a single maximum vibration frequency is insufficient to account for the variation of specific heat, as might be expected in accordance with Born and Kármán's theory, which takes into account the structure of the crystal unit.

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* This point is being subjected to further scrutiny in the cases of mercury and zinc.

That there is a relation between frequency and mass in accordance with the periodic law is well known; mass enters into all the formulæ for the frequency since they are all based on the simple pendulum formula $\nu=\frac{1}{2\pi}\sqrt{\frac{F}{M}}$ but none of the theoretical formulæ are completely supported by the results of specific heat determinations. Only at very low temperatures should one expect the characteristic frequencies calculated from the elastic constants to agree perfectly with those calculated from specific heat data. Accurate data for the elastic constants at such temperatures are not, however, available (*vide* Jeans "Report on Radiation and Quantum Theory," 2nd edition, 1924).

If $\beta\nu$ is plotted against $\log M$ (*see* diagram), similar elements tend to fall on straight lines to a common point, whereabouts Hg, Pb, Bi and Tl are situated (elements with high atomic weight and low melting point). A relationship of this sort implies a rather complicated connexion between melting point and mass, for if $\log M=a+b\nu$ where a and b are constants and $\nu=K\sqrt{\frac{T_s}{MV^{2/3}}}$,

$$T_s=\frac{1}{b^2K}\cdot MV^{2/3}\,.\,(a^2-2a\log M+\log M^2).$$

It is noteworthy, however, that agreement appears to be closer for the most reliable values of $\beta\nu$ obtained from specific heat data than for those calculated from the melting-point formula (e.g., Cd, Zn and Ag, Cu).

TABLE I.—*Group* 1*a*.

	Chem. Const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	*Cp—Cv*† 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Li	—0·327	13·0	179	6·0	0·33	508	418	(330) Dewar, (450) Koref, Laemmel, &c.
Na	0·453	23·8	97·9	7·2	0·5	207	171	125 Günther, (160) E. and R.,* Dewar, 170 Griffiths, Koref, &c.
K	0·798	45·5	63·5	8·3	0·52	122	99	(70) E. and R., (100) Dewar.
Rb	1·309	56·2	39·0	9·0	0·60	74	60	...
Cs	1·596	71·0	28·5	9·7	0·68	54	43	...

* Eastman and Rodebush.

† *See also* Eastman, Williams and Young (loc. cit.).

The specific heat measurements vary too much amongst themselves to show any conclusions as to the relationships between the values of the frequency for this group of elements (*see*, however, the diagram).

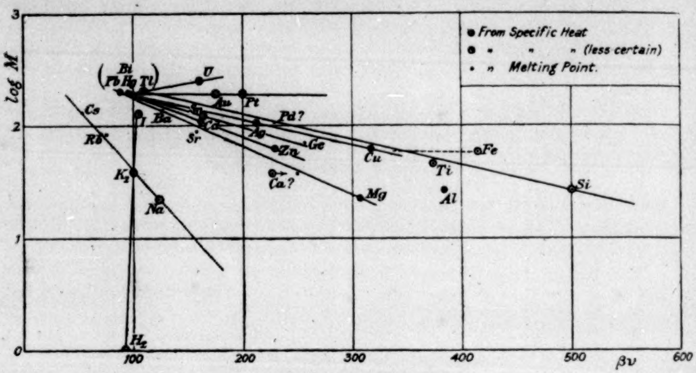

TABLE II.—*Group* 1*b*.

	Chem. const.	Atomic volume.	M. Pt. °C.	α × 10⁻⁵	*Cp—Cv* 20°C.	βν 1	βν 2	βν obs.
Cu	1·116	7·12	1,083	1·628	0·166	356	324	315 Keesom and Onnes, Nernst, Griffiths, &c.
Ag	1·460	10·3	960·5	0·763	0·24	248	223	215 Nernst, Koref, Griffiths, &c.
Au	1·853	10·2	1,063	1·36	0·20	178	177	(175) Dewar, Richards and Jackson.

The results for silver (215) and copper (315) are known with considerable certainty. A line drawn through these points passes through the mercury point. The value for gold (175) would need to be halved to fall on this line.

TABLE III.—*Group* 2*a*.

	Chem. const.	Atomic volume.	M. Pt. °C.	α × 10⁻⁵	*Cp—Cv* 20°C.	βν 1	βν 2	βν obs.
Be	**0·156**	**4·9**	1,278	...	...	**1,143**	...	**(800) Dewar, (960) Humpidge, Nilson, &c.**
Mg	**0·490**	**14·0**	**650**	**2·47**	**0·20**	**379**	**318**	**(305) Nernst and Schwers, Eastman and Rodebush.**
Ca	**0·815**	**25·8**	**804**	**1·68**	**0·08**	**260**	...	**226 Günther.**
Sr	**1·325**	**34·5**	**(800)**	...	...	**159**	...	...
Ba	**1·618**	**38·2**	**(850)**	...	...	**125**	...	**(100) Dewar.**

The same remarks apply as for Group 1a. The value of $\beta\nu$ for magnesium 305 appears to be fairly well fixed by the measurements of Nernst and Schwers, Eastman and Rodebush, and others, but the value for calcium is open to doubt. Rodebush has shown that two forms exist with a transition point about 400 °C. It is likely that the value $\beta\nu=226$ is a mean of a lower value and a higher value (about 250).

TABLE IV.—*Group 2b.*

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	*Cp*—*Cv* 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Zinc	1·134	9·2	419·4	2·83	0·28	230	222	228 Nernst.
Cadmium	1·487	13·0	320·9	2·88	0·25	151	150	168 Rodebush, Griffiths, Ewald, Behn, Barschall.
Mercury	1·864	14·2	—38·89	2·96	...	66	107	96 Simon.

The values of $\beta\nu$ obtained from the specific heat measurements for these three metals fall close to a straight line. (The value for cadmium is perhaps a little too large; the recent measurements of Rodebush taken alone lead to a value $\beta\nu=163$).

TABLE V.—*Group 3a* .

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	*Cp*—*Cv* 2 °C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
B	—0·038	6·3	2,200	...	...	1,505	...	(1,130) Koref.
Al	0·56	10·0	660·0	2·184	0·23	404	384	(386) Nernst and Schwers and others.

The values for scandium and the rare earth metals are not known, so that no relationship can be established between the frequencies in this group. The value for Boron is doubtful; Dewar's measurement indicates a lower figure.

TABLE VI.—*Group 3b.*

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	*Cp*—*Cv* 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Ga	1·176	11·8	29·75	...	...	136	...	...
In	1·501	15·8	154	4·0	...	114	118	...
Tl	1·877	17·2	301·5	3·0	0·30	96	99	(99) Nernst and Schwers (Ewald, &c.)

In this group thallium is the only element for which the specific heats have been accurately determined. The values obtained from the melting point formula for this group appear to agree well with those observed (e.g., Al and Tl). The $\beta\nu$ values for Ga, In, Tl lie close about a straight line.

TABLE VII.—*Group 4a.*

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	*Cp—Cv* 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
C	0·030	3·42	...	...	...	...	...	1,850 Nernst.
Si	0·583	12·0	1,413	...	...	502	...	(500) Nernst and Schwers.
Ti	0·931	10·6	(1,800)	...	...	444	...	(375) Dewar.
Zr	1·350	...	(1,530)	...	...	355	...	(225) Dewar.
Hf	...	...	...	...	...	...	...	...
Th	1·960	...	(1,690)	...	...	156	..	(110) Dewar.

In this group the values of $\beta\nu$ from the specific heat data are unreliable, except in the case of diamond and silicon. The value for silicon obtained from measurements above 100°C. is higher than the value given above (Richards and Jackson's and Russell's results give values over 600; Russell giving different values for two forms of silicon). The value of $\beta\nu$ depends on the state of the element.

TABLE VIII.—*Group 4b.*

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	*Cp—Cv* 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Ge	1·200	13·5	958·5	...	...	258	...	(280 ?) (Nilson Petterson).
Sn	1·575	16·3	231·8	2·14	0·25	120	157	(166) Rodebush, 198 (white) 253 (grey) Brönsted.
Pb	1·886	18·3	326·9	2·76	0·43	96	101	88 Nernst, Onnes and others.

The specific heat of germanium not being known accurately it is not possible to draw any conclusions from this set of elements. Another difficulty is the allotropy of tin. Bronsted gives the values 198 for white and 253 for grey tin, but more recent specific heat measurements indicate a lower value for white tin. A line passing through lead and tin (166) passes near the point 258, calculated from the melting point of germanium.

TABLE IX.—*Group 5.*

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	*Cp—Cv* 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
P	(0·749)	17·0	44·1	...	...	184	...	420 Ewald.
As	(1·223)	13·1	...	...	...	232	221	(295) Ewald.
Sb	(1·539)	18·3	630·5	0·976	0·05	15·3	...	(140) Günther.
Bi	(1·891)	21·4	271	1·26	0·09	98	(161)	(100 ?) Dewar, Richards and Jackson.

The values for these elements are also doubtful. Ewald's value does not quite agree with Dewar's for white phosphorus or for bismuth. Gunther's value for antimony is very considerably less than those obtained from the work of Dewar, Ewald, &c., which indicate a value obout 190. The value for bismuth is also uncertain.

TABLE X.

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp-Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
V	0·972	9·1	(1,720)	...	...	444	...	...
Nb	1·364	7·35	(1,950)	...	...	372	...	...
Ta	1·799	10·9	(2,800)	0·65	...	275	263	(270) Siemens and others.

The frequency determined from the melting point gives approximately a straight line for these elements, but there exist no satisfactory specific heat data.

TABLE XI.—*Group* 6.

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp-Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs
S	(0·670)	15·5	(112·8)	7·0	...	206	170	(280) Dewar. (170-430) Nernst, Koref.
Se	(1·259)	16·5	220	...	...	145	126	(190) Dewar.
Te	(1·569)	20·6	(453)	...	...	129	...	(120) Tilden, (150) Dewar.

For sulphur Nernst gave $Cv=\frac{1}{4}F(74)+\frac{3}{4}F(510)$; it is probable the specific heats of selenium and tellurium are also dependent on more than a single frequency maximum.

TABLE XII.

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 10^{-5}$	$Cp-Cv$ 2 °C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Cr	0·935	7·76	(1,550)	0·824	0·036	443	490	(460) Dewar, Richards and Jackson.
Mo	1·384	9·4	(2,500)	0·506	0·03	377	434	(300) Dewar, ? Richards and Jackson.
W	1·808	9·65	(3,370)	0·445	0·05	322	329	(275) Dewar.
Ur	1·976	12·7	(1,350)	...	...	166	...	(160) Dewar.

For this set of elements the values determined from the specific heat data are only approximate values, but they agree fairly well with the melting point formula, except in the case of molybdenum. The values of $\beta\nu$ from the melting point formula for Cr, Mo, W fall on a straight line (uranium is not on this line).

TABLE XIII.—*Group* 7.

	Chem. const.	Atomic volume.	M.Pt. °C.	α $\times 10^{-5}$.	$Cp—Cv$ 2 °C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
H_2	—1·584	(13·1)	—259	...	...	...	...	91 Simon.
F	0·329	(12·4) ?	—223	...	...	78	...	...
Cl	0·736	(16·2) ?	—101	...	...	129	...	...
Br	1·265	(19·2) ?	—7·2	...	...	100	...	...
I	1·566	25·7	113	(8·4)	...	87	66	106 Nernst and Schwers.

It is possible that the values of $\beta\nu$ for these elements are nearly the same (*see* diagram).

	Chem. const.	Atomic volume.	M. Pt. °C.	α $\times 1\ ^{-5}$.	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.
Mn	1·021	7·54	(1,264)	2·22	0·35	399	295	(320).

(Dewar's results give $\beta\nu=340$, but results of Richards and others indicate a value about 300.)

TABLE XIV.—*Group* 8.

	Chem. const.	Atomic volume.	M.Pt. °C.	α $\times 10^{-5}$.	$Cp—Cv$ 20°C.	$\beta\nu$ 1	$\beta\nu$ 2	$\beta\nu$ obs.	Dewar.
Iron ...	1·031	7·11	1,552	1·1	0·1	438	417	(415) Ewald, Günther, Richards, Behn.	(380)
Cobalt ...	1·067	6·70	1,490	1·234	0·13	429	394	(360) Richards, Tilden	(345)
Nickel ...	1·064	6·68	1,452	1·23	0·16	426	393	(345) Richards, Schimpf, Tilden, Behn.	(345)
Ruthenium	1·422	8·3	(1,950)	0·96	...	342	317	...	(360)
Rhodium	1·429	8·5	1·970	0·85	...	269	264	...	(320)
Palladium	1·453	9·3	1,555	1·17	0·21	292	270	(230) Richards, Behn.	(255)
Osmium	1·832	8·5	(2,500)	0·65	...	277	279	...	(235)
Iridium ...	1·840	8·63	2,355	0·649	0·076	266	266	(240) Behn.	(265)
Platinum...	1·847	9·13	1,755	0·893	0·117	229	230	(200) Richards, Behn Tilden.	(210)

Even in the case of iron the specific heat measurements lead to values of $\beta\nu$ varying from 360-460. The latest measurements of Günther at low temperatures give a value about 453. Nothing very definite is available as to the frequencies of these elements.

The lighter elements Li, Be, B, C appear to have frequencies slightly more than double those which would be necessary for them to lie on the lines on which the rest of their group resides.

The evidence is insufficient to draw any definite conclusion yet as to the relationships between the characteristic temperatures of the elements, and in many cases a single frequency is not applicable. All that can be said is that in certain cases $\beta\nu$ can be expressed as a straight line function of the logarithm of mass, and that there is a general tendency for a function of this sort to apply.

DISCUSSION.

The PRESIDENT said that the Author had collected a mass of evidence linking up experimental results with theoretical formulæ in a very interesting way. His Paper dealt with subject matter of a kind not often brought before the Society, and would doubtless stimulate others to undertake research on the numerous unsettled questions he had touched upon.

Prof. H. S. ALLEN (communicated): As I have pointed out in a Paper on "Atomic Frequency" (Phil. Mag., Vol. 34, page 478, 1917), later writers seem to have overlooked the important work of Sutherland on the subject of atomic vibrations in solids. As long ago as 1890 he claimed to have found simple relations between the *periods* of vibration of elements at the melting-point for several chemical families. It is a characteristic feature of Sutherland's theory of fusion that melting occurs when the space between the molecules attains a certain value relatively to the size of the molecules. Thus αT_s is constant, a relation afterwards verified by Gruneisen. Sutherland's theory leads directly to Lindemann's formula for the atomic frequency at the melting-point.

With our present knowledge of the structure of crystalline solids it seems essential that in future investigations of atomic frequencies the arrangement of the atoms in a space lattice should be considered. The lattice theory of atomic heats due to Born and Kármán certainly presents great difficulties as regards calculation, but it must be a much closer representation of the actual conditions than the earlier theories.

VII.—THE SENSIBILITY OF CIRCULAR DIAPHRAGMS FOR THE RECEPTION OF SOUNDS IN WATER.

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Received September 14, 1924.

ABSTRACT.

The Paper describes an investigation of the sensibility of circular diaphragms as receivers of submarine sounds. The diaphragms were mounted with one face immersed in water and to the other was attached a microphonic or electro-magnetic detector. The diaphragms were all designed to have a frequency of 850 ~ under these conditions.

The response of the diaphragms to sounds of a single definite frequency was measured and resonance curves were obtained from which the magnitude of the damping due to the detector was determined. A value was also obtained for the concentration of energy in the restricted volume of the experimental tank, and an investigation was made of the effect of the proximity of sound reflectors and of the vibrating diaphragm of the source of sound.

It was shown that when corrections were made for these various effects, the values for the resonance amplitude and for the persistence of vibration were consistent with those determined mathematically by Prof. H. Lamb for diaphragms under ideal conditions.

The investigation was extended to cover complex sounds or "noises" of no definite pitch, and the results were again consistent with theoretical anticipations.

IN a previous communication* an account is given of an investigation of the natural frequency of circular diaphragms when vibrating under different conditions in air and water. The object of the present research has been to extend this investigation to the sensibility of these diaphragms when used to detect sounds in water, and to determine the law governing the amplitude of vibration of diaphragms when actuated by sound waves in water.

The mathematical treatment of the subject is again due to Prof. H. Lamb, and may be summarised briefly as follows :—

The displacement x of the centre of a circular diaphragm, radius a, vibrating with one side in contact with water, is subject to an equation of the form

$$A\ddot{x}+B\dot{x}+Cx=P\cos pt \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

where A is a factor equal to the effective mass of the diaphragm, including the additional inertia due to the movement of the water in the vicinity of the diaphragm. B is a factor representing the damping of the diaphragm which is supposed to result entirely from the radiation of energy to the water in the form of sound waves. C represents the restoring force acting on the diaphragm corresponding to unit displacement of the diaphragm from its equilibrium position.

The numerical values of these factors can be deduced from equations (5), (26) and (4) respectively in Prof. Lamb's Paper on "The Vibration of an Elastic Plate in Contact with Water."†

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* Proc. Phys. Soc., Vol. 35, April (1923), "On the Frequency of Vibration of Circular Diaphragms."

† Proc. Roy. Soc., A, Vol. 98, p. 205 (1920).

P is the value of the total pressure due to the sound waves on the diaphragm, and $n=\frac{p}{2\pi}$ is the applied frequency.

This gives for the maximum displacement of the centre of the diaphragm

$$D=\frac{P}{\sqrt{(C-p^2A)^2+p^2B^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In the case of resonance, the natural frequency of the diaphragm n_0 is equal to that of the sound waves acting upon it.* Then

$$p_0=2\pi n_0=\sqrt{\frac{C}{A}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$D_{\text{max.}} \text{ being equal to } \frac{P}{p_0B}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If now p_0 is regarded as a constant, B varies as a^4 and P may be assumed proportional to the area of the diaphragm, i.e., varying as a^2.

$$D_{\text{max.}}\ \alpha\ \frac{a^2}{a^4}, \text{ i.e., as } \frac{1}{a^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Therefore, for diaphragms having the same frequency in water, the amplitude at resonance should be inversely proportional to the square of the radius.

The value of $D_{\text{max.}}$ is also independent of A. Therefore, the mass of the diaphragm has no influence on the maximum amplitude at resonance. If a diaphragm is loaded by a mass M at its centre, the quantity A becomes $A+M$, and D is correspondingly less at frequencies away from the resonance point. Fig. 1 is a theoretical amplitude-frequency curve calculated for diaphragms 3 in. in diameter, frequency 850 ~ in water, with and without a 20-gramme load at the centre. The resonance curve is much sharper in the case of the loaded diaphragm, but the maximum amplitude remains the same.

The amplitude of a damped vibrating diaphragm diminishes according to the law $e^{-\alpha t}$,

$$\text{where } \alpha=\frac{5}{36}\ \frac{\rho}{\rho_1}\ \frac{p^2a^2}{(1+\beta)\,hc} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \dagger(6)$$

The quantity α is numerically equal to the ratio of the damping factor to the mass factor, i.e., to the quantity B/A.

When a diaphragm is responding to a source of sound of variable frequency, the energy of resonance falls to one half its maximum value when the frequency of the sounder differs from that of the diaphragm by the fraction α/p, i.e., if the amplitude at resonance is reduced to half value by reducing the frequency z vibrations, then

$$\alpha=\frac{B}{A}=2\pi z \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where the value of B is that for resonance. This resonance value of B is always assumed throughout the remainder of this Paper.

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* The damping, though considerable, is not sufficient to affect the frequency appreciably. Lamb, loc. cit.

† Lamb, *ibid.*, equation (29).

The object of this research was therefore to confirm these theoretical deductions and to examine the shape of the experimental resonance curves in order to determine from them the most sensitive type of diaphragm for the reception both of pure notes and for general sounds. In most practical cases a detector, usually of the microphone type, must be attached to the diaphragm, and this will modify both A and B. Experiments were therefore made to determine the loading and damping effects of detectors on the sensitiveness of a diaphragm.

All the experiments were carried out in a tank 12 ft. long, 3 ft. wide, and 2½ ft. deep, the sides of which being of brick, coated with a smooth surface of tar, acted as sound reflectors. The assumption that the energy is dissipated from a vibrating diaphragm in the form of semicircular waves was therefore not applicable in the case, and in the following experiments the effect of this concentration of energy on the value of B was actually measured.

An investigation was also made of the results produced by various local conditions on the value of B, including the effects of the proximity of the wall of the tank, and of the vibrating diaphragm of the sounder.

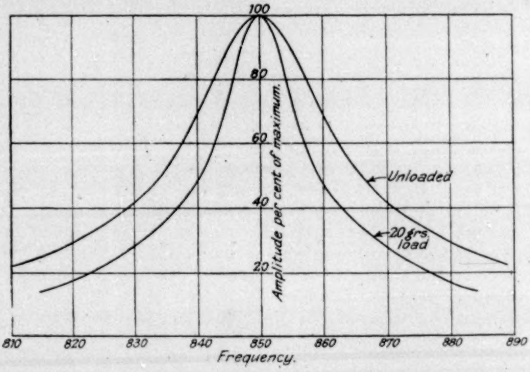


Fig. 1.—Theoretical Resonance Curve for 3 in. Steel Diaphragms (Loaded and Unloaded, Frequency 850 ∿ in Water). No Additional Damping.

Experimental Methods.

The diaphragms which were used in connexion with this investigation were all of the same type as those used in the earlier investigations of the frequency, and a series was constructed all having the same fundamental frequency when vibrating with one side in contact with water. To avoid accidental effects all the experiments were repeated with a duplicate set of diaphragms. The frequency selected was 900 vibrations per second, and the actual frequencies of the diaphragms usually lay well within 5 per cent. when loaded with their respective detectors. This load (kept constant during the experiment) caused a slight lowering of the fundamental frequency, which was now about 850 ∿.

The general description of the diaphragms and the method of supporting them in their massive steel holder has already been described in the Paper referred to previously.

The sounds employed to excite the diaphragms were of two main classes—(*a*) a pure note of a frequency which could be varied continuously from 100 to 2,000 or more vibrations per second, and which was used more particularly for the investigation of the diaphragms in their resonance region; and (*b*) complex sounds, the constituents of which were in general different from the fundamental frequency of the diaphragm. For the investigation of the response of the diaphragms to complex sounds, a source emitting simultaneously sounds of all frequencies from 0 to 2,000 ∿, and of equal intensity, would have been most desirable, but in its absence, different sources giving out a certain amount of general noise were employed for this purpose. The sources of complex sound which were principally used were as follows:—A clock lying at the bottom of a deep vessel floating in the water; a motor running on the wall of the tank with an attachment to throw it out of balance and to produce a loud rattling sound; an electric fan on the wall of the tank; a pendulum which was allowed to fall from a fixed height and strike the wall of the tank; and a stream of air bubbles delivered from a tube immersed in the water of the tank.

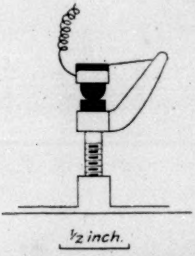


FIG. 2.—SIMPLE CARBON CONTACT MICROPHONE.

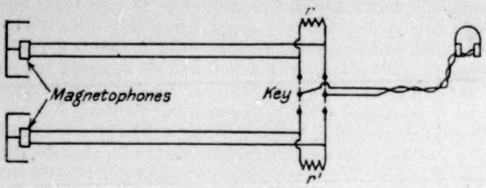
The resultant vibrations of the diaphragms were detected by the following means:—(*a*) A "Brown" telephone (or magnetophone), the reed of which was attached to the centre of the diaphragm; (*b*) a granular microphone of the standard type made by the Automatic Telephone Manufacturing Co.; (*c*) a small form of simple carbon contact microphone (S.C.C.)—Fig. 2—constructed specially for these experiments.

METHOD OF COMPARING DIAPHRAGMS.

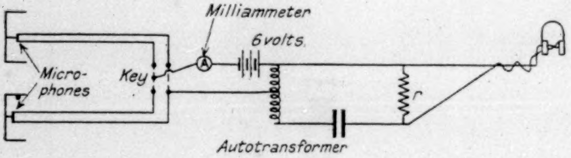
Fig. 3, (*a*) and (*b*), shows the method of comparing the diaphragms. The two diaphragms were suspended in the water equidistant from the source of sound, and the detectors connected in turn to the listening telephone. A shunt was introduced into the circuit of the more sensitive diaphragm and was adjusted until the sounds heard were equal or, alternatively, similar shunts were introduced into both circuits and adjusted to extinguish the sound heard altogether.

From the values of these shunts the ratio of the loudness of the sound heard in the two cases is readily calculable.*

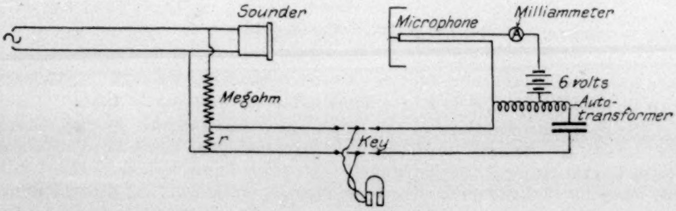
The ratio so obtained involves the constants of the detectors, and if the latter


r
Magnetophones
Key
r'

(*a*) *Shunts r r' adjusted till sounds are equal or extinguished (Magnetophone detector)*


Milliammeter
6 volts
Micro-
phones
Key
A
r
Autotransformer

(*b*) *Shunt r adjusted to extinguish sound. (Microphone detector)*


Sounder
Microphone
Milliammeter
A
6 volts
Megohm
Auto-
transformer
r
Key

(*c*) *Shunt r adjusted till sounds are equal.*

FIG. 3.—METHOD OF COMPARING DIAPHRAGM SENSIBILITY.

are then interchanged and their constants not altered in the process, it can be shown that the true ratio is the geometric mean of the two ratios obtained as above.

A method of measuring numerically the loudness produced in the listening telephone is illustrated in Fig. 3 (*c*) and consists in comparing the loudness with that produced by a portion of the sounder current tapped off as in a potentiometer.

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* *See* V. O. Knudsen, Phys. Rev., Vol. 21, No. 1 (1923). Also A. Stefanini, N. Cimento 26, Oct.-Dec. (1923).

It was not possible to obtain an absolute measure of the amplitude of the diaphragm, since the energy supplied to the telephone depends on the constants of the detector, but relative values were obtained by transferring the same detector to another standard diaphragm.

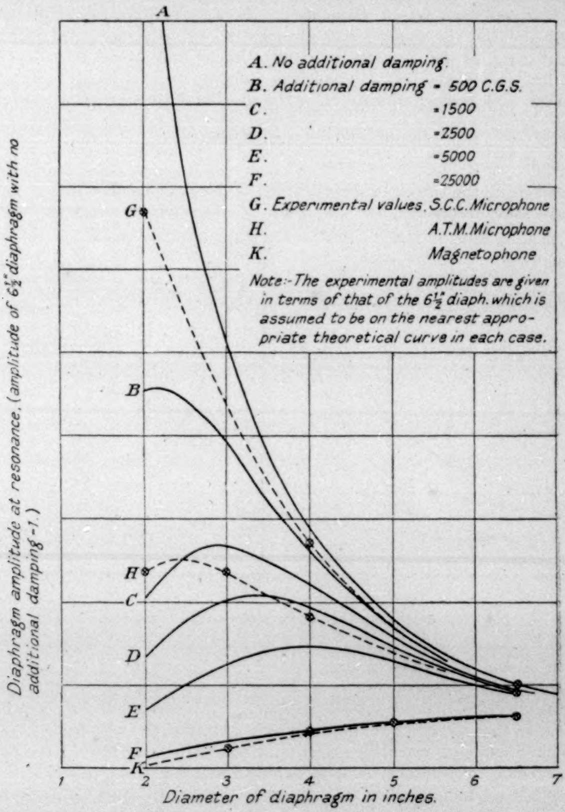


FIG. 4.—VARIATION OF SENSIBILITY WITH ADDITIONAL DAMPING.

The condition that the constants of the detector should not alter in the process of transference was difficult to obtain, and a considerable number of repetitions were necessary to obtain reliable average results. In the case of microphone detectors the resistance and sensitiveness vary with handling, or when the current is made or broken. These difficulties were partly reduced by bringing the resistance and current always to a definite value before taking an observation.

In all experiments where resonance curves were plotted, the diaphragms were held vertically and the sounder was placed with its diaphragm also vertical, at a constant distance of 6 ft. from it, and always in the same position in the tank.

The loudness of the sound heard was found to be very nearly uniform at all points in the tank, except within 1 ft. of the sounder diaphragm or near the walls.

EXPERIMENTAL RESULTS.

Pure Sounds—Sensibility at Resonance.

According to the theory of the vibration of diaphragms of different dimensions, but of the same frequency, the amplitude of vibration at resonance for a given excitation should be inversely proportional to the square of the radius of the diaphragm.

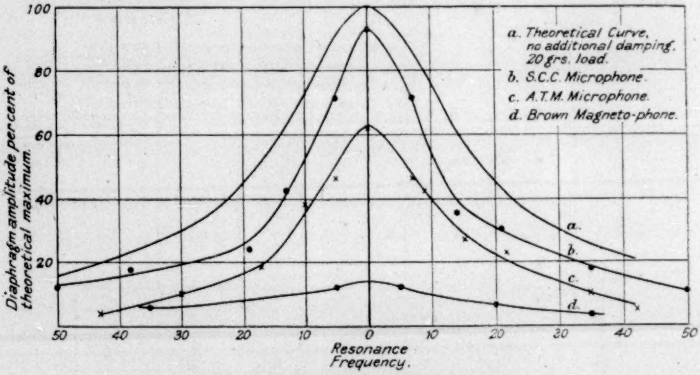


FIG. 5.—RESONANCE CURVES FOR 4-IN. DIAPHRAGM.

The results of experiments made to examine this deduction are given in Fig. 4. It will be seen that according to the theoretical deduction the amplitude at resonance of the 2 in. diaphragm should be about ten times as great as that of the 6½ in. diaphragm, whereas the results show it to be only about twice as loud when the A.T.M. microphone is used as detector, and about six times with the S.C.C. microphone.

This difference can be readily accounted for by the fact that the simple theory takes no account of the extra damping due principally to the damping effect of the microphone which has to be added to that due to the water in contact with the diaphragm. It will be seen by comparing the typical theoretical and experimental resonance curves, Fig. 5, that, though the curves have a close similarity of shape, the breadth of the experimental curves is different from that of the theoretical. The " breadth " of each curve was readily measurable, and the quantity " z " (equation 7) determined in each case, from which the value of the ratio B/A was calculable.

In order to account for the difference in the theoretical and experimental values of B/A, the possible causes of variation of the two factors were examined.

The value of A was modified to correct for the additional mass of the detector, as in the earlier calculations on the frequency of circular diaphragms in which this factor was involved. As these figures were found to agree closely with those determined experimentally, this value of A was considered as being correct.

The factor B may be modified either by additional damping, due to the detector, which increases its magnitude, or by the concentration of the energy of the sound waves in the tank, which reduces it.

These modifications were investigated in detail experimentally.

(1) *Damping Due to Detector.*—The damping of the vibrations of a diaphragm due to the attachment of the microphone was very marked. If a diaphragm were placed horizontally, it was found that any loose particles such as globules of mercury, drops of water or metal filings produced the same effect, and in an actual experiment a quantity of fine steel filings sufficient to fill three standard A.T.M. microphones, lying loosely on a small plate attached to the diaphragm at its centre, reduced the height of the resonance peak of a 4 in. diaphragm to one-third (using an A.T.M. microphone as detector).

In the case of the Brown telephone detector there was considerable electromagnetic damping (*see* Fig. 4). In order to reduce the microphone damping to a minimum, the special type of small single carbon contact (S.C.C.) microphone was devised (*see* Fig. 2), which consisted of a small block of carbon with a convex surface mounted on a pivoted lever (a fine steel needle), in contact with a flat block of carbon fixed rigidly to the centre of the diaphragm; the total weight of the whole microphone including the screw for attachment was barely 2 grammes, and its measured damping effect was about one-tenth that of an A.T.M. microphone.

Comparative resonance curves were obtained with these two types of microphone and with the Brown magnetophone, examples of which are given in Fig. 5 for a 4 in. diaphragm, and which show the relative damping effects very clearly. The slight variations in the mass of the detectors produce small variations in the breadth of the curves, but have no effect on the total height of the resonance peak.

It was also observed in the course of these experiments that detectors having loose mechanical parts produced very marked damping effects—and in particular devices of this kind designed on the lever principle to give greater amplitude to the microphone did not produce the desired magnification.

(2) *Energy Concentration.*—Owing to the fact that the diaphragm did not form part of an infinite rigid wall, and was not immersed in an infinite volume of water, the energy loss was very much smaller than calculated theoretically. Energy loss due to radiation of sound energy to the air, and to heating of the metal of the diaphragm was relatively negligible.

From the figures obtained for the breadth and height of the resonance curves it was found that the concentration effect was constant, and that the value B/A was reduced to one-half of its normal value, provided the diaphragm was not within a few inches of the wall of the tank, or in the vicinity of another vibrating diaphragm (e.g., the sounder diaphragm itself) or any sound absorber such as an air cavity. Care was taken in all measurements to ensure that this was the case. Fig. 6 shows the result of an experiment in which the value of B/A was measured when a 2 in. diaphragm was brought close up to the sounder diaphragm, the two diaphragms being kept parallel. The increase in the damping was very marked at distances less than one foot, and was accompanied by a lowering in the pitch and amplitude. The proximity of a rigid wall tended to sharpen the maximum but the effect was not marked.

It was also observed that by suitably adjusting the position of a vibrating diaphragm, the damping of the sounder diaphragm could be increased or diminished, and its loudness diminished or increased accordingly.

In making the measurements of the breadth of resonances the diaphragms were fitted with a collar consisting of a circular sheet of iron ⅛ in. thick, forming the base of a hollow cylindrical vessel in the centre of which the diaphragm was mounted, so as to lie flush with the collar. This collar was found to make no appreciable difference to the breadth of resonance in any but the case of the 6½ in. diaphragms, where it increased the breadth by about 25 per cent., which would be anticipated, owing to the absence of a flat solid rim in this type of diaphragm.

A set of numerical values for the various factors is given in the following table, in which theoretical and experimental values are compared :—

Diam. of Diaph.	Type of micro-phone.	Theoretical values.			Assumed value of microphone damping, b.	$\frac{B+b}{2\cdot1\times A}$.	Experi-mental value.	Relative amplitude at resonance.	
		B (resonance).	A	$\frac{B}{A}$				Theotl.	Exptl.
16·5 cms (6½″)	A.T.M.	78,000	374	208	5,000	105·8	103·2	1·0	1·0
10·15 cms. (4″)	A.T.M.	10,300	73·2	141	5,000	99·6	104·1	2·05	1·8
7·61 cms. (3″)	A.T.M.	3,260	27·8	96·6	5,000	141·5	131·8	2·14	2·4
16·5 cms. (6½″)	S.C.C.	78,000	374	208	500	100·0	93·2	1·0	1·0
10·15 cms. (4″)	S.C.C.	10,300	73·2	141	500	70·5	76·9	2·74	2·7
5·08 cms. (2″)	S.C.C.	655	8·2	78·5	500	66·3	62·8	6·46	6·7

Frequency of the diaphragm 850 ∾ ; all units c.g.s.

In the above table the values of A, B and B/A are obtained theoretically as described earlier in this Paper ; the quantity b, proportional to the damping due to the detector is added to B and its magnitude is such that the new values of $(B+b)/A$ are proportional to the experimental values of $2\pi z$.

The ratio of these values therefore gives the relation between the actual values of the energy dissipated in the water of a tank of finite dimensions, and the theoretical values in open water. It will be seen that this factor is almost exactly $1/2 \cdot 1$, and is constant for all diaphragms and detectors.

The *true* value for the microphone damping is therefore equal to $b/2 \cdot 1$.

The experimental values of B/A have all been corrected for the load due to the detector.

As a check on the accuracy of the value of $B+b$ the new amplitude of vibration was calculated in each case, assuming the amplitude to be proportional to $a^2/B+b$ (equation (4)). These values are given taking the amplitude of the $6\frac{1}{2}$ in. diaphragm

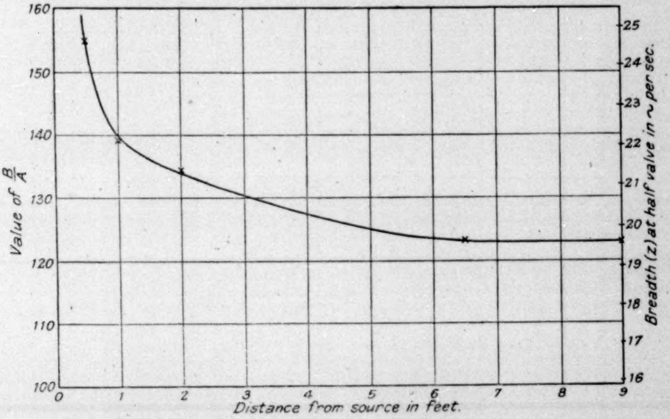


FIG. 6.—VARIATION IN THE BREADTH OF RESONANCE CURVE OF 2-IN. DIAPHRAGM WHEN APPROACHING SOUNDER DIAPHRAGM.

as unity for each detector, and the close agreement with the experimental values confirms the accuracy of the assumption.

In Fig. 4 is given a series of curves showing the effect of the damping due to the detector on the amplitude of vibration of different diaphragms, under the experimental conditions, the measured values being also plotted on the same graph. These show that the maximum sensibility is obtained when the microphone damping is equal to the water damping—and that in the case of an A.T.M. microphone, this damping is about equal to that of a $3\frac{1}{4}$ in. diaphragm with water on one side. This result is confirmed by the theoretical deductions of Mr. H. W. Hilliar, who carried out a mathematical investigation (unpublished) of the various damping effects on the vibration of a diaphragm, and is corroborated by actual experience at sea, where the " water " damping would be 2·1 times greater and the most sensitive diaphragm therefore 2·8 in. in diameter.

COMPLEX SOUNDS.

In the case of a noise consisting of sounds of various frequencies, in general away from the resonance region, the value of B is relatively insignificant and the maximum sensitivity is governed by the value of A. Here again the smaller diaphragms are more sensitive, but when a load is applied at the centre, the maximum sensibility occurs when the added load is equal to the effective mass A. The total added load in these experiments (except in the case of the 6½ in. diaphragm, when the effect was relatively small) amounted to 20 grammes—roughly one-fifth the mass of a 3 in. steel diaphragm, which should therefore be the most sensitive type for use with this load.

This conclusion was confirmed by experiments, the results of which are given in Fig. 7. In considering these values it is important to notice that some of these sounds, particularly those due to the tapping and to the bursting bubbles were in the nature of "shocks," and that therefore on the cessation of the impulse the diaphragm resounded with its own frequency, hence in considering this "after-ring" the case of pure resonance arises again rather than that of forced vibration. This after-ring was observed to be much more persistent with a large diaphragm, and almost absent with a small one, thus confirming the excessive damping effect of the detector in the latter case. It should be remarked, however, that these noises,

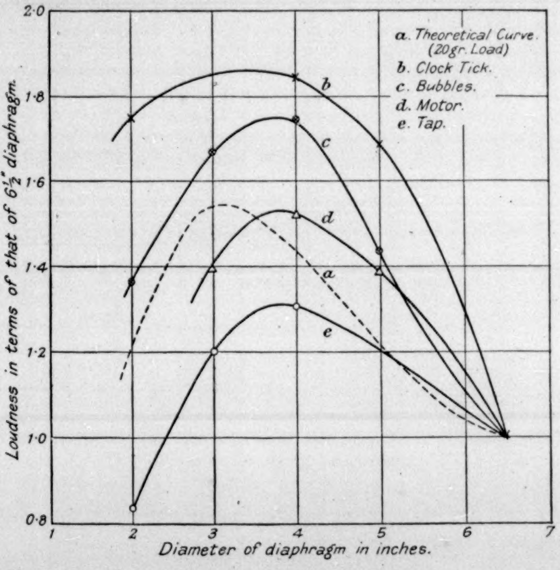


FIG. 7.—VARIATION OF SENSIBILITY TO COMPLEX SOUND. (A.T.M. MICROPHONE DETECTOR.)

though not true "complex-sounds," give a very good imitation of the sound produced by a vessel and since the ultimate object of the investigation was to produce the diaphragm most sensitive to sounds of this nature, the results obtained were of direct practical value.

This research was carried out during 1916 under the personal direction of Sir E. Rutherford, to whom the author has great pleasure in expressing his indebtedness.

The author also wishes to thank Dr. J. H. T. Roberts for his valuable collaboration in the experimental portion of this research.

This Paper is now published by permission of the Admiralty.

DISCUSSION.

Dr. F. L. **Hopwood** said that the Paper, which dealt with work carried out during the war but not previously released for publication, revived old memories, pleasant and otherwise. The author who had worked in a tank would have preferred the open sea, but workers in the open sea expressed a preference for tanks, while the speaker's lot had fallen to a reservoir, which combined the disadvantages of both. At the beginning of the war the experimental diaphragms ranged from 2 in. to 2 ft. in diameter, and the available theory indicated that the amplitude under the conditions discussed would increase indefinitely with decreasing diameter. Thin diaphragms in which the overtones were excited had also been tried. The damping produced by the detector was accounted for by the energy absorbed in the receiving system. The credit for finding the real optimum diameter was due to the work of the author and Prof. Lamb. No other authors have dealt with this subject in English, but the findings given in the Paper are supported by the German work "Unterwasser Schalltechnik."

Sir **Richard Paget** said that, like the previous speaker, he was moved to a reminiscent frame of mind by the Paper. Some idea of the progress made could be gathered from the fact that before the war the diaphragms which were used in connection with submarine bells used to be tuned *in air* to the required pitch ; the water damping would then put them out of tune by as much, possibly, as an octave. It was interesting to compare the diaphragms discussed with those found in Nature, in the ears of men and animals ; the diaphragm of an ear is of much smaller diameter, and the damping is much smaller than that due to a microphonic detector. It might be interesting to experiment with an unloaded diaphragm free from damping other than radiation damping ; this could be done by employing optical methods of detection. Prof. Rankine and others had very successfully used the human ear as a subaqueous detector.

Capt. C. W. **Hume** asked how far it was justifiable to express the damping due to the detector as an increment in the coefficient B in the resistance term of a differential equation for damped forced oscillations. The damping due to "loose mechanical parts" or to loose particles, to which the author refers in part (1) of his paragraph on this subject, would be rather in the nature of a succession of blows than a continuous function of $\dot{x}$, and it was difficult to believe that a granule inertia microphone would offer a resistance proportional to the velocity of the diaphragm. It was noteworthy that the curve for a "simple carbon contact" seemed to resemble the theoretical curve in shape far more closely than did the other curves.

Prof. A. O. **Rankine** said he ought to explain that the idea of using the human ear as a subaqueous detector had originated with Sir Richard Paget, who conducted his investigations while suspended in the sea by his feet. The speaker had preferred to adopt the more dignified posture normal to the human species. He did not consider the ear very sensitive to subaqueous sounds, but its damping was small in comparison with the damping due to the water.

Dr. E. H. **Rayner** said that the use of subaqueous bells had been mentioned. It is generally stated that the bells do not give out the notes they are supposed to give, the pitch being somewhat indefinite. How is it, therefore, that a diaphragm can be tuned to respond selectively to a given bell ?

Author's reply :—(To Dr. Hopwood) : The energy absorbed by the detector is undoubtedly used in exciting the listening telephone. The most sensitive microphone, therefore, produces the greatest damping effect. In the case of microphones this energy merely serves as a "trigger" for the release of a much greater amount of energy from a battery. In the case of the magnetophone, on the other hand, the whole of the energy is derived from the diaphragm, and for this reason the magnetophone damping is excessive, although it is a much less sensitive detector than the microphone. No attempt was made to study the case of overtones, since it was found that the frequencies of the overtones of the diaphragms examined were not in agreement with Prof. Lamb's theory, probably on account of the presence of the boss and detector at the centre. In practice it was found that hydrophones with thin diaphragms in which the overtones were excited could not be reproduced in large numbers with any consistency, and thicker diaphragms were accordingly substituted in which the fundamental tone predominated. I have read the book referred to, which is by Lichte, and it was reading it which led me to communicate this Paper, even though the work described is eight years old, as it did not seem desirable that the only published experimental work on this subject should be in German. I have made no reference to it in the Paper, since this work was naturally quite independent, nor does the German work possess priority.

(To Sir Richard Paget): An optical method similar to the one suggested has actually been used to examine the vibration of diaphragms. The work was done in 1919 by Dr. A. B. Wood, but is unpublished. His results are fully in agreement with those quoted in this Paper.

(To Capt. Hume): The microphone depends for its action on the variation in the pressure on the carbon granules, which is never zero, since there is never a complete break in the circuit; the damping is therefore continuous throughout the whole cycle. In the case of the S.C.C. microphone, when the vibration of the diaphragm becomes very violent, "chattering" of the contact begins, but this does not occur with small amplitudes. Mr. H. W. Hilliar, in the investigation referred to in the Paper, deals with the theory of damping very fully, and compares the microphone to an arrangement of carbon blocks which slide over one another frictionally, and his conclusions are in complete agreement with experiment.

(To Dr. Rayner): The "bells" referred to in this paper are the standard Trinity House bells used for submarine signalling. They are specially tuned to a definite pitch in water, and it was found that diaphragms could readily be tuned to this frequency. A full description of these bells is given by Dr. Drysdale in the chapter on Submarine Signalling in "The Mechanical Properties of Fluids," where a drawing is also given.

VIII.—A DIRECT-READING FREQUENCY METER OF LONG RANGE.

BY ALBERT CAMPBELL, *B.A.*

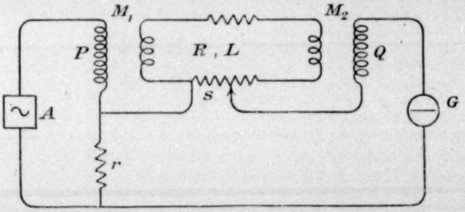
Received November 21, 1924.

ABSTRACT.

A direct-reading frequency meter for audio frequencies is described. It is a null instrument, reading by single adjustment, the working system embodying a new method of balancing mutual inductance by resistance. The standard type has five ranges, covering from 180 to 4,000 ∼ per second, with accuracy of the order of 1 in 1 000, and negligible temperature coefficient except at the lowest frequencies; an extra set of ranges from 18 ∼ per second upwards can be easily added.

THE accurate measurement of audio frequency is of importance, not only in acoustical experiments, but also in a great variety of electrical measurements; and it is often a most convenient method of determining speed of rotation in mechanical tests.

Perhaps the simplest method of measuring electrical frequency is the author's "wave-form sifter,"* in which a mutual inductance M is balanced against a capacitance C, or his Bridge Sifter, almost as simple, but better in working, in which a similar balance is obtained. But these methods, valuable as they are in many cases, have certain inherent disadvantages. For example :—



(*a*) Even the best condensers show considerable variation of capacitance with frequency and temperature; in the less costly types these defects may be accentuated.

(*b*) The condenser-mutual inductance (C, M) system does not easily lend itself to the design of an instrument with a number of ranges.

In spite of these disadvantages the author has found it possible to design C, M instruments which work well, but a better solution of the problem is based on a novel method of balancing mutual inductance against resistance. This is shown in the figure.

Two mutual inductances, M_1 and M_2, are connected with an alternating source A and a detector G (e.g., a vibration galvanometer or telephone) as shown, their secondary circuits, with added resistance, forming a loop having total self-inductance L and resistance R, of which a portion s is tapped off into the detector circuit; r is a non-inductive resistance. Let pulsatance $\omega=2\pi n$, where n is the frequency of the source. Then, when G shows no current,

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* Proc. Phys. Soc., Vol. 21, p. 69 (1907), and Vol. 24, p. 107 (1912).

$$\omega^2 M_1 M_2 = Rr \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$M_1 s = Lr \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If we fix M_1, r and L we can set s once for all so as to satisfy condition (2). Then for any given frequency condition (1) can be satisfied by adjusting only M_2. Then

$$\omega^2 = Rr/M_1 M_2,$$

or

$$n \propto 1/\sqrt{M_2} = a/\sqrt{M_2}.$$

If r and M_1 are invariable, by changing only R we can obtain various values of the scale multiplier a.

In the actual instrument the scale of the variable M_2 is marked to read n directly for a given value of R, making $a=1$, and a number of other ranges are obtained by altering R in suitable proportions, giving simple scale multipliers.

It will be noticed that, unlike the C, M sifter types, which require double adjustment, the instrument reads by *single adjustment*.

If the resistance r has residual self-inductance l, and if there is direct mutual inductance m between P and Q, the conditions of balance become

$$\omega^2[M_1 M_2 - L(l-m)] = Rr \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and

$$M_1 s = Lr + (l-m)R. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Now l is usually negligible; if not, it can be balanced by an equal small value of m.

In the instrument m can be set to zero, but as this may be altered by the effect of leads or other external circuits, a very minute adjustment is left for m, so that perfect sharpness of balance can always be ensured.

In the standard type of instrument the scale is so designed as to give an accuracy of reading of at least 0·25 mm. for 1 part in 1,000 (of n) at all points. There are five ranges (scale multipliers 0·3, 0·5, 1, 2 and 3), giving a total range of about 180 to 4,000 ∿ per second.

For the ranges from 600 up to 4,000 ∿ per second the temperature coefficient is negligible. It is very small for the 300 to 600 range, and slightly more for the lowest range. For the ranges from 600 to 2,500 ∿ per second the actual accuracy is in general within 1 in 1,000.

In the same instrument, by suitable variation of r and s an additional series of ranges can be provided, covering from 18 to 400 ∿ per second, the detecting instrument being a vibration galvanometer for the lower frequencies.

DISCUSSION.

Dr. E. H. RAYNER said it gave him great pleasure to hear once more from the Author, who had sought peace and quiet in Ireland after doing very much to put in order, in this country, those standards of measurement with which he was uniquely qualified to deal. He suggested that a letter of appreciation should be forwarded on behalf of the Fellows present. The precise measurement of audio-frequency, no less than that of radio-frequency, was becoming of the greatest importance in modern industry. In the United States the frequency of the supply mains was used for time-keeping, and consumers were able to obtain very small neat clocks run as synchronous motors from the mains. The frequency was kept constant by the aid of a chronometric pointer at the generating station, a second pointer driven by a synchronous motor being mounted coaxially with this. The supply was controlled manually, so as to keep the second pointer in step with the first, giving a frequency of 50·000 ∿.

Dr. D. OWEN also congratulated the Author on his method. He had himself some years ago explored the possibilities of bridge methods of measuring frequency, but had failed to hit on a really satisfactory one. Mr. Campbell has achieved a fine success in the present meter. The two conditions of balance are quite independent, and the values of mutual inductance and resistance required are well within reasonable practical limits. For measurements on an impure source a thermionic oscillator might be desirable as an intermediary, using the beat method.

The PRESIDENT said he felt sure the Author would have been glad of a constant-frequency supply, such as that described by Dr. Rayner, had this been available in the laboratory which he had built (and plastered with his own hands) in Ireland. However, he had contrived to make time measurements to an accuracy of 0·01 per cent. with an antiquated grandfather clock. With the consent of those present, he would ask the Secretary to send a letter to the Author as Dr. Rayner had suggested. (Applause.)

Dr. ALFRED HAY (communicated): I wish to express my appreciation of Mr. Campbell's ingenious application of the principle of balancing a mutual inductance against a resistance to the construction of a new type of frequency meter. It is interesting to know that perfect sharpness of balance can always be ensured; this presumably applies only to practically pure sine waves, as it would appear that with a distorted wave silence could never be obtained. The new instrument appears to be capable of a very high degree of accuracy. Perhaps Mr. Campbell would state whether there is any likelihood of the adaptation of his instrument, with a suitable vibration galvanometer, to the measurement of low frequencies—from, say, 12 to 100. In my experience, available commercial instruments for the measurement of such frequencies are unsatisfactory.

Mr. S. BUTTERWORTH (communicated): The bridge recommended by the Author for the measurement of frequency is probably the best that could be devised. In addition to its long range, the second balance, which is always necessary in alternating current bridges, can be made to hold for all frequencies, so that in actual use only a single setting is necessary. This is a very great advantage. The Author makes no remark on the effect of want of quadrature in the mutuals. This will affect both equations of balance, and though unimportant at the lower frequencies, may be appreciable when the frequency is as high as 4,000 per second. The difficulty could be avoided by calibrating the system by known frequencies instead of relying on the electrical equation of balance. A more serious effect of the want of quadrature would, however, be a lack of sharpness of balance at the higher frequencies, as the second balance is not quite independent of frequency when the impurity of the mutuals is taken into account. It is possible, however, that by a judicious choice of the mutuals this effect might be made negligible, as the two impurities can exert mutually cancelling effects in the equation of balance.

Dr. E. T. PARIS (communicated): The direct-reading frequency meter described by Mr. Campbell appears to be an instrument which should have many applications to acoustical experiments. The range mentioned in the Paper for the standard type of instrument is 180 to 4,000 ∿ per second, and while this should be sufficient for most needs, there are some branches of acoustical work in which the frequencies to be measured are generally below 180 ∿ per second. It would be of interest to know if there is likely to be any difficulty in the manufacture and operation of a meter of this type for a range of (say) 50 to 100 ∿ per second.

Mr. B. DAVIES (communicated): The first production to issue over the threshold of Mr. Albert Campbell's private laboratory is a novel frequency meter. One is glad to see it. Like all his work at the N.P.L., and that of his colleagues who have been responsible for the remarkable development of standardization in this country, this meter goes forth with the usual high credentials. To dispose of variable inductance or variable capacitance or both as the basis of frequency measurement is a great step forward and a very neat one, eliminating as it does obvious inherent difficulties as well as difficulties in manufacture. It seems to me it were well if Mr. Campbell allowed two types of his instrument to find their way to the market. One, a standard type of high accuracy in tune with Mr. Campbell's own heart, and another, at a lower price, possessing moderate accuracy, to be used as a hack instrument in the rough and tumble work that goes on in most laboratories for purpose of approximate computations. This latter, it is well understood, is against Mr. Campbell's grain ; but if he can be prevailed upon to consider this heretical suggestion from the point of view of the "common weal," whose interests he has persistently at heart, we may witness a concession ! There is a demand for both types.

Mr. ROLLO APPLEYARD (communicated) : To realise the importance of Mr. Albert Campbell's results it has to be remembered that the direct measurement of frequency within the required range necessitated the invention of a method, and—what in some respects was more difficult—the design and construction of an instrument capable of satisfying the conditions that govern such determinations. His success in this two-fold task will be appreciated by all physicists. In net-works of the kind involved in this problem, the conditions for balance have always to be represented by two equations, relating respectively to the "real" and to the "imaginary" parts of the solution ; and corresponding to these equations there must be two adjustments. The advantage of the net-work devised by Mr. Campbell is that the two equations are simple, and that one of them may be satisfied by a permanent adjustment. He thus fulfils, without complexity, the direct-reading conditions. Another method of measuring frequency and of relating the other factors in the network may be sought by adjusting them to give no current in the second mesh (of *RL*), instead of no current in the third (of *QG*). The required conditions for no current in the second mesh I find to be

$$M_1 L_3 \omega^2 = r_1 s \quad \text{. . . . .} \quad (1B)$$

and

$$M_1(r + R_3 + s) = rM_2 \quad \text{. . . . .} \quad (2B)$$

where L_3, R_3 denote the self-inductance and resistance of the third mesh. The relations of (1B) and (2B) may enable check-readings to be obtained by transferring *G* from the third to the second mesh. Also (2B) expresses the ratio of the two mutual inductances in terms of resistances for the case when (1B) holds. On the other hand, for the purpose contemplated, it might imply a permanent adjustment between M_1 and M_2, and in that event it would lead to a less convenient arrangement than that embodied in Mr. Campbell's instrument.

AUTHOR'S reply (communicated) :—

(To Dr. Alfred Hay and Dr. E. T. Paris) : The standard type of the instrument is designed to cover the range of ordinary telephonic frequencies. By a slight elaboration it is easy to furnish in the same instrument a series of lower ranges from 18 up to 400 ∿ per sec. It is only necessary to reduce *r* and *s* in the same proportion (here 100 : 1). These lower ranges have been found to work very well with a vibration galvanometer as detector.

(To Mr. S. Butterworth) : In the design of the instrument the possibility of errors at the higher frequencies due to impurity in the mutual inductances was carefully kept in mind. Careful tests have shown that with coils of fairly simple type the actual errors lie within about 1 part in 1,000 up to frequencies of 3,200 ∿ per sec. If specially high ranges are wanted, the impurities in the inductances can be reduced by subdividing the coils in any of the well-known ways. In general the instrument follows the equation of balance so accurately that it can be accurately calibrated without the use of any known frequencies.

THE PHYSICAL SOCIETY OF LONDON
AND
THE ROYAL METEOROLOGICAL SOCIETY

A DISCUSSION
ON
Ionization in the Atmosphere and its Influence on the Propagation of Wireless Signals

Held November 28, 1924

AT THE
IMPERIAL COLLEGE OF SCIENCE, SOUTH
KENSINGTON, S.W.

LONDON:
THE FLEETWAY PRESS, LTD.,
DANE STREET HOLBORN, W.C.1.

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Opening Address by Dr. W. H. ECCLES, *F.R.S.*

MODERN wireless signalling came into being when Oliver Lodge showed the British Association at Oxford in 1894 that dots and dashes could be carried by Hertzian waves across a distance of 150 yards and received by a coherer which was automatically re-set after each response. In the following year the Russian physicist, Popoff, used a long vertical wire and a smaller automatic coherer for recording lightning flashes. Thus the warning that atmospheric electricity was going to interfere with the new-born art of wireless telegraphy followed quickly upon its inception. Before another year was over Rutherford had sent signals half-a-mile across Cambridge, and Marconi had improved and invented many pieces of apparatus for making electric waves and receiving them, and had developed a complete plant for wireless telegraphy that already showed commercial possibilities. But he also quickly found that lightning flashes, near and distant, produced records on his morse tape and sometimes obliterated messages. These unwanted signals produced by natural electric waves were called X's or strays. They have also been called atmospherics, statics, sturbs and other names. They are still with us and are still a terrible nuisance, although nowadays our apparatus is less susceptible to them. They can often be heard in the telephones of broadcast listeners where they appear sometimes as sharp clicks, sometimes as grinding or crashing noises, and sometimes as frying or hissing sounds. They are distinctly connected with meteorological conditions, and therefore fall within the scope of this evening's discussion.

Within two or three years of the events just mentioned, Marconi had sent messages to distances of 20 or even 40 miles in spite of intervening hills. Did the electric waves go through or over the hills ? Captain Jackson* (now Admiral Sir Henry Jackson) settled this question by comparing signals received in a ship close to a mountainous island with those received well beyond. He found there was a distinct shadow which proved that the waves went not through but over the hills by the process of bending called diffraction.

But when in 1900 Marconi sent signals across the Atlantic, thus overleaping a hill of salt water 130 miles high, it was felt that something more than diffraction was required to explain this event. Heaviside's † suggestion that the waves might be guided round the globe by a conducting layer in the upper atmosphere seemed very promising, though there were many objections to it. This ad hoc layer was given the name of the Heaviside layer and produced a crop of speculation. For instance, Erskine Murray‡ pointed out that the wave front from a sending station would spread over the globe as an expanding ring until its diameter was 8,000 miles, and then would travel onwards as a contracting ring until it reached the Antipodes, when, if the absorption had been small, the signals would be very strong indeed. But speculation was much disturbed by Marconi's* discovery in 1902 that a ship crossing the Atlantic could receive signals four or five times as strong by night as by day. The Heaviside hypothesis could not deal with this new surprise.

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* Proc. Roy. Soc., Feb. (1902).

† Ency. Britt., 10th edition, Vol. 33, page 215.

‡ Inst. Ell. Engineers, Dec. (1905).

It was obvious, however, that the daily increase of opacity might be connected with ionization of the air by the suns rays. A gas is said to be ionized when it contains electrified particles of molecular or smaller order of magnitude. The smallest ions are particles of negative electricity devoid of matter and are called electrons; the next smallest are single atoms or molecules of gas carrying positive or negative charges of electricity. The question arose how could the presence of these ions in air affect the propagation of electric signal waves? No clear suggestion was made until 1912,† when it was shown by mathematical reasoning that an electric wave moving through air containing ions makes the ions vibrate with the result that the velocity of propagation is increased—somewhat as waves in water have their velocity altered by the presence of fine mud in suspension or of innumerable minute air bubbles. An immediate deduction from the known fact that the ionization of the air is greater in the upper strata than in the lower is that the waves will follow downward bending trajectories rather like rifle bullets. Evidently there will be two sharply marked cases, one in which the ions are electrons, and the other in which they are of molecular size and therefore several thousand times heavier. It was suggested in 1912 that the Heaviside layer was a layer so high and rarefied that free electrons could exist in it and that the absorption of energy was negligible, that its lower surface was fairly sharply defined, and that the layer therefore bent electric waves of all lengths downwards so sharply as to simulate the act of reflection from a mirror. Accepting all this the waves from a sending station would run under the dome formed by this permanently ionized layer much as sound waves run under the dome of St. Paul's from the Whispering Gallery. It was also suggested that in contrast with the Heaviside layer certain layers underneath it became ionized daily by the sunlight, and that as these ions are relatively heavy the bending of the wave trajectory would depend upon the wavelength and show daily variations. It ought to be mentioned here that Fleming‡ has suggested that the variation of inductivity of the air with height might be an aid or an alternative to ionic refraction.

As information accumulated during the succeeding years these hypotheses of the effect of the ionization of the atmosphere received increasing confirmation. For instance, as was pointed out§ in 1920, in the rarefied atmosphere where electrons exist and the mean free path is great, the plane of polarization of electric waves will be rotated because the agitated electrons move in the earth's magnetic field. To this cause may be attributed the errors that arise in applying direction finding apparatus by night. Again, the recent successes of amateurs in communicating with Australia and New Zealand with short waves during certain of the dark hours affords additional support. The fact that many of the successful amateurs utilize a series condenser in their transmitting antenna in order to avail themselves of the upwardly directed radiation so obtained has been frequently emphasised at recent meetings of the Radio Society of Great Britain and is corroboration of the existence of the Heaviside layer. Again, experimenters in England and France have deliberately aimed waves at the sky by means of parabolic reflectors and have been enabled to transmit to great distances. It seems not unlikely that very short waves, less than 10 metres long, may prove to be of great practical value by day as well as by night in the early future. But all this confirmatory information would not be satisfying if it were not for the fact that other branches of meteorological and geophysical science are bringing independent evidence of the existence of these conducting layers in the atmosphere. To some of this evidence, as well as to other radio telegraphic evidence, we are gathered to listen this evening.

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* Proc. Roy. Soc., June (1902).

† Proc. Roy. Soc., June (1912).

‡ Proc. Phys. Soc., August (1914).

§ Inst. Ell. Engineers, December (1920).

ATMOSPHERIC IONIZATION AND ITS VARIATIONS.

By C. Chree, *Sc.D., LL.D., F.R.S.*

Abstract.

The atmospheric electricity elements dealt with include the potential gradient, the numbers of positive and negative ions and the charges carried by them, the conductivity of the air and the vertical air-earth current. For instantaneous values it suffices to know the potential gradient and either the conductivity or the current. But in the case of mean values, depending on a number of individual observations, a knowledge of all three elements is necessary. Particulars are given of the annual variations observed at various stations. Information as to diurnal variation is very scanty, except in the case of potential gradient. The monthly values in the annual variation, and the hourly values in the daily variation, are expressed as percentages of their arithmetic means. This is done partly because less uncertainty attaches to relative than to absolute values in the case of all the electrical elements. Reference is made to some conclusions as to the diurnal variation of potential gradient and the existence of a sun-spot period recently arrived at by Dr. Bauer and Dr. Mauchly, of the Carnegie Institution of Washington.

§ 1.

THE fact that the earth's atmosphere is ionized was really discovered before anything was known about ions. It was long supposed that the loss experienced by all charged bodies arose from defective insulation in the supports, but eventually it was discovered that defective insulation accounted for only part, sometimes a very small part, of the loss. For historical details the recent work by B. Chauveau* may be consulted. I have derived considerable assistance from Parts II and III of this work in making the following compilation.

Amongst the earlier investigators Elster and Geitel—regarded in their day as the Castor and Pollux of atmospheric electricity—were pre-eminent for their zeal. They devised apparatus for measuring "dissipation," as it was called, and the radioactive contents of the atmosphere. Most of the earlier observations were made with their instruments. The results obtained, unfortunately, are difficult to interpret, and no use is made of them here. Most of the results now available as to the ionic contents of the atmosphere have been obtained with the Ebert apparatus, which is in a convenient form for use. Here, again, unfortunately some suspicion attaches to the results. The instrument was accused a good many years ago by Prof. W. F. G. Swann† of underestimating the number of negative ions. Observations and experiments made by E. H. Nichols‡ appeared to show that if the defect described by Prof. Swann existed, it was trifling under the ordinary conditions of the Kew observations. Prof. Swann adhered, however, to his original views, and they seem to derive support from a more recent investigation by H. Norinder.§

The ions which the Ebert apparatus is primarily intended to catch are those now known as light ions, with mobilities in the neighbourhood of 1·5 (i.e., under a potential gradient of 1 volt per centimetre they have a velocity of about 1·5 cm. per second). If it is assumed that all the ions caught are of one species, a second observation of a modified kind with the Ebert apparatus enables the mobility to be calculated. What the first observation really gives is the charge of free electricity —positive or negative as desired—per cm.^{3} of the atmosphere on ions caught by the instrument. Assuming each ion to carry a definite charge, the number of ions positive or negative can be calculated. In the earlier observations taken with the apparatus the ionic charge was assigned the value $3{\cdot}4\times10^{-10}$ E.S.U. originally found by Sir J. J. Thomson. The value obtained more recently by Prof. Millikan, $4{\cdot}8\times10^{-10}$ E.S.U., has now come into general use. It is not always easy to ascertain definitely what value has been used.

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* Electricité Atmospherique, Paris Libraire Octave Doin (1924).

† Terrestrial Magnetism, Vol. 19, p. 205.

‡ Terrestrial Magnetism, Vol. 21, p. 87.

§ Stockholm Arkiv För Mathematik, Astronomi och Fysik, Band 15, No. 2.

§ 2.

It is now known that the heavy ions discovered by Langevin may have to be taken into account. Our information about them is largely dependent on observations made in Ireland by McClelland, Kennedy, Nolan and others. According to the Irish experiments, the mobility of these heavy ions is only about $\frac{1}{3000}$ of that of the light ions, and if they were not more numerous than the light ions their influence on the conductivity would be negligible. They appear, however, often to be much more numerous. For the mean numbers n_1 and n_2 of the positive and negative light ions per cm.^{3} observed at a number of stations, Chauveau* gives

$$\text{On land, } n_1=750,\ n_2=630,\ q'\equiv n_1/n_2=1{\cdot}20.$$
$$\text{At sea, } n_1=730,\ n_2=580,\ q'=1{\cdot}25.$$

But at Dublin McClelland and Kennedy† found the number of heavy ions to vary between 3,700 and 60,000 per cm.^{3}, the mean number being about 16,000.

The Ebert apparatus, as ordinarily used, catches only a small percentage of the heavy ions; but when they are as numerous as at Dublin, even a small percentage becomes important. At Dalkey, in the neighbourhood of Dublin, McClelland and Kennedy found usually less than 1,000 heavy ions per cm.^{3}. They also found the number of light ions in Dublin to be small compared with the number at Dalkey. The presumption is that the number of heavy ions is largely determined by the amount of dirt in the atmosphere. If light ions ceased to be light ions by simply attaching themselves to dust, unless dust particles are all of one size, we should expect the resulting heavy ions to have a variety of mobilities. Pollock‡ claimed to have observed at Sydney ions of mobility intermediate between the ordinary light and heavy ions. But the heavy ions observed in the natural atmosphere at Dublin make apparently at least an approach to uniformity. In any case, what seems to happen is that in the dirt-laden air of large towns there is a smaller number than usual of light ions. A larger share of the vertical current, which is continually passing between the earth and the upper atmosphere, has to be carried by heavy ions of low mobility. The electrical conductivity falls, and the potential gradient goes up. In a recent letter to "Nature,"§ Prof. Nolan illustrated the rise in the potential gradient observed in a number of cases at Dublin with increase in the number of heavy ions. The fact has been observed at a good many stations that potential gradient tends to be higher on dusty or hazy than on clear days. This question was investigated in considerable detail some years ago at Kew Observatory,* use being made of records of visibility. With the setting up at Kew of an Owens' atmospheric-pollution recorder in 1921, a more precise investigation became possible. The practice at Kew has been to measure the potential gradient at each hour on 10 selected fine-weather days each month. Dividing these days for each month of the years 1921 and 1922 into five days of less and five days of greater atmospheric pollution, the following mean results were obtained† :—

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* l.c. Troisieme Fascicule, p. 89.
† Proc. Roy. Irish Acad., Vol. 30, Section A, p. 72, &c.
‡ Proc. Roy. Soc., N.S.W., p. 61, &c.
§ Nature, Vol. 113, p. 493 (1924).

Clean days.	Dirty days.
Dirt, 0·252 milligram per metre^3.	Dirt, 0·490 milligram per metre^3.
Potential gradient, 252 v./m.	Potential gradient, 343 v./m.

The inference was drawn that if the effect on the potential gradient were a linear function of the dirt, then, if the atmosphere had been perfectly clean, the mean potential gradient for the two years would have been only about 150 v./m., instead of about 300 v./m. as observed. Along with the rise in potential there went a fall in conductivity,‡ but the information on this point is much less complete.

Part only of the dust in the atmosphere is the work of man. In some places, and at some seasons, Nature is a great dust producer. Still, the fact remains that if the stations where atmospheric electricity results have been obtained had all been in the country, instead of being mostly urban or suburban, our information would have been more truly representative than it is.

§ 3.

Returning to instrumental questions, it may be added that most of the results at sea have been obtained from observations made with special apparatus on board the magnetic survey vessels of the Carnegie Institution of Washington.§ On land use has also been made of an apparatus designed by Dr. C. T. R. Wilson.‖ With it direct measurements can be taken of the potential gradient and the air-earth current, whence the conductivity can be calculated. A variant of this apparatus was used by Lutz¶ at Munich.

In the case of the Ebert apparatus the positive and negative ions and the conductivities for which they are responsible are measured separately. The free charges, positive and negative, per cubic *metre* corresponding to Chauveau's mean land values $n_1=750$, $n_2=630$ are respectively

$$\begin{aligned} E_1 &= 750\times10^6\times4\cdot8\times10^{-10}=0\cdot36 \text{ E.S.U.} \\ E_2 &= 0\cdot30. \end{aligned}$$

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* Proc. Roy. Soc., A, Vol. 95, p. 210.

† Proc. Roy. Soc., A, Vol. 105, pp. 321 and 323.

‡ cf. Nature, Vol. 113, p. 856 (unchanged or diminished current, when gradient is higher, implies lower conductivity).

§ Carnegie Institution of Washington, Dept. Terrestrial Magnetism, Researches, Vol. 3, pp. 361 *et seq.*

‖ Proc. Camb. Phil. Soc., Vol. 13, p. 184.

¶ Sitzungsberichte, K. Bayer., Akad. der Wiss. Math.-physik Klasse, Jakrgang, 1911, p. 329.

If we prefer electromagnetic units, and charges per cm.^{3}, we have

$$E_1=1{\cdot}2\times10^{-16} \text{ coulombs per cm.}^3.$$

If n_1, n_2 be the numbers, k_1 and k_2 the mobilities of the free positive and negative ions, all supposed of one class, the air-earth current, assuming the air itself at rest, is given by $i=(k_1n_1+k_2n_2)eP$, where e is the ionic charge and P the potential gradient (measured, of course, in the same units as the other quantities).

As an example, suppose

$$n_1=n_2=500,$$
$$k_1=k_2=1{\cdot}5 \text{ cm./sec. for 1 v./cm.},$$
$$P=100 \text{ v./m.}$$

Using E.M.U. throughout, we have

$$k_1=k_2=1{\cdot}5\times10^{-8},$$
$$P=(100/100)\times10^8,$$
$$e=1{\cdot}6\times10^{-20},$$

and so $i=1{,}000\times1{\cdot}5\times1{\cdot}6\times10^{-20}=2{\cdot}4\times10^{-17}$, or $2{\cdot}4\times10^{-16}$ amp. per cm.^{2}. As an example of electrostatic units, taking the data as above, we have

$$k_1=450, \text{ as the volt is } 1/300 \text{ of the electrostatic unit},$$
$$e=4{\cdot}8\times10^{-10},$$

and the conductivity λ_1 from positive ions is given by

$$\lambda_1=450\times500\times4{\cdot}8\times10^{-10}=1{\cdot}1\times10^{-4} \text{ E.S.U.}$$

§ 4.

Table I. contains a variety of results for the annual variation, and Table II. a few data for diurnal variation. The monthly and hourly values are shown as percentages of their mean. This is done partly with a view to bringing out resemblances or differences between the several elements and the different stations, partly because percentage values may remain correct and useful even when large uncertainties attach to absolute values. For example, in the case of P, so long as the factor for reduction to the infinite plane is constant, the percentage values are unaffected by an error in the factor.

Thanks, in the first instance, to Lord Kelvin's invention of the water-dropping electrograph, most stations concerned with atmospheric electricity have a continuous record of potential gradient, and most of the data for P in Table I. are derived from electrographs. Potsdam and Davos (Switzerland) are the only two stations which had continuous—or quasi-continuous—records of any other element. The apparatus in use at both stations gives a measure of the conductivities λ_1 and λ_2 due to positive and to negative ions separately, by measuring the loss of charge in a body charged alternately positive and negative. When the conductivity and potential gradient are known, the air-earth current can be calculated. The earlier of the two sets of data for Potsdam were derived by K. Kähler[*] from 12 months, October, 1910, to September, 1911. The later refer to the years 1912 to 1921, and are derived from a recent Paper by H. Markgraf.[†] The results for Davos are for the year December,

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[*] Ergebnisse der Meteorolog. Beob. in Potsdam, im Jahre 1911, p. xvi.

[†] Meteorologische Zeitschrift, Vol. 41, p. 65.

TABLE I.—*Annual Variation of Electrical Elements (Percentages of Annual Mean).*

Place.	Element.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Annual Mean.
Kew (A) ...	P	**145**	131	122	106	82	60	**59**	60	75	96	128	136	333 v/m
(B) ...	P	147	139	119	102	72	**46**	53	53	73	100	**160**	138	299 v/m
,, ...	E_1	**72**	81	98	93	118	**135**	114	134	108	96	79	**72**	0·155 E.S.U. (per m^3)
,, ...	E_2	**68**	76	103	105	118	126	117	**138**	107	79	85	74	0·114 E.S.U. (per m^3)
,, ...	i	82	89	96	105	110	107	99	111	123	**129**	**74**	75	$0{\cdot}78\times10^{-16}$ amp/cm^2
(C) ...	P	**165**	143	107	81	58	53	**44**	82	80	101	125	**165**	255 v/m
,, ...	E_1+E_2	70	50	80	125	123	126	**182**	135	115	81	67	**46**	0·37 E.S.U. (per m^3)
,, ...	λ	44	58	74	128	133	140	**173**	147	106	90	63	**43**	$0{\cdot}27\times10^{-4}$ E.S.U.
,, ...	i	97	102	**84**	104	100	90	104	**115**	108	110	100	85	$0{\cdot}54\times10^{-16}$ amp/cm^2
Tortosa	P	133	96	112	91	92	**80**	84	93	92	100	112	**116**	102 v/m
	λ	97	102	105	**107**	100	100	99	96	104	102	96	**92**	$1{\cdot}80\times10^{-4}$ E.S.U.
	i	**122**	108	110	101	87	**78**	82	85	99	100	111	118	$2{\cdot}03\times10^{-16}$ amp/cm^2
Potsdam (A)	P	136	110	98	111	89	**69**	71	93	72	99	109	**144**	254 v/m
,,	λ	**74**	81	109	102	114	119	**121**	102	107	97	89	84	$0{\cdot}95\times10^{-4}$ E.S.U.
,,	i	101	89	105	110	101	89	89	97	**84**	105	101	**127**	$2{\cdot}37\times10^{-16}$ amp/cm^2
,,	λ_2	**70**	82	111	98	109	123	**125**	105	109	98	89	82	$0{\cdot}44\times10^{-4}$ E.S.U.
,,	i_2	98	89	110	107	98	92	92	98	**86**	107	101	**122**	$1{\cdot}09\times10^{-16}$ amp/cm^2
(B)	P	**146**	143	107	92	79	75	**72**	74	79	87	109	137	202 v/m
,,	λ_2	90	**75**	99	107	112	107	**115**	**115**	104	106	92	78	$0{\cdot}45\times10^{-4}$ E.S.U.
,,	i_2	**135**	102	104	100	93	**85**	92	**85**	90	97	104	114	$0{\cdot}98\times10^{-16}$ amp/cm^2
Davos	P	**152**	140	136	102	88	**52**	71	73	74	71	98	146	69 v/m
	λ	**58**	81	77	96	109	**140**	115	121	118	127	99	61	$2{\cdot}68\times10^{-4}$ E.S.U.
	i	87	**125**	109	110	108	**80**	90	98	95	101	105	93	$1{\cdot}71\times10^{-16}$ amp/cm^2
Munich	P	**157**	131	140	60	49	**48**	84	82	84	64	154	146	1·88 v/m
	i	**159**	128	102	86	89	68	**65**	88	119	77	94	128	$1{\cdot}02\times10^{-16}$ amp/cm^2
Strelitz	E_1	88	**84**	100	105	90	96	107	**112**	111	111	105	91	0·340 E.S.U. (per m^3)
	E_2	85	**83**	105	111	88	97	105	**113**	110	109	104	89	0·327 E.S.U. (per m^3)

1909, to November, 1910. The original discussion by Prof. C. Dorno[*] I have only seen in abstract; the data given here are derived from a review of Dorno's work by K. Kähler.[†]

The results as to charges, conductivities and current at the other stations are from eye readings and refer only to a short period of the day, about 15 h. at Kew, 11 h. at Tortosa, and noon at Munich and Strelitz. The hour of observation was presumably dictated by the general convenience, and there is no reason to suppose that the values observed represent the mean value for the day.

In the case of Kew the (A) values for P are from hourly curve readings on fine weather days, for 11 years 1912 to 1923. The (B) data, on the other hand, all refer to 15 h., when P is normally below its mean value. The results for P are from the selected fine weather days, 10 a month. The other results are derived from all the days of absolute observation, representing on the average about half the days of the year, no observations being taken when the weather is wet or very stormy. The Kew (C) results are from data given in a Paper by G. Dobson.[‡] These results, including those for P, are from eye observations with the Wilson apparatus near 15 h. during the years 1909 to 1912. The results for i for August, 1911, were abnormal and have been omitted, as this had apparently been done by Dobson.

The Tortosa[§] results are means from the years 1918 to 1923. The mean value of P for the day from the years in question differed very little from that at 11 h. The values obtained for λ and i at Tortosa were derived by the Gockel-Schering method. The results for Munich, due to Lutz,[||] represent the results of eye readings on fine weather days from an instrument similar in type to Dr. Wilson's. The results for Strelitz are from a recent Paper by S. Wiedenhoff.[¶] They represent three years' observations, extending from August, 1920, to July, 1923.

In Table I. the highest and lowest values are in heavy type. The annual variation of P is conspicuous at all the stations; P is high in winter and low in summer. The ionic charges and the conductivity vary on the whole in the opposite direction to P, being highest in summer. At Tortosa, however, the conductivity seems little dependent on the season, and consequently the vertical current shows a large fluctuation similar to that in P, the highest values occurring in winter. A similar phenomenon appears at Munich, and also in the (B) results from Potsdam for i_2 (i.e., the current carried by the negative ions). In the (A) results for Potsdam, the mean values for i_2 and i from the four summer months May to August, are only slightly below the means for the twelve months; while at Kew, in both groups of years, the mean value of i for the four summer months is somewhat above the mean for the year.

How far the diversity in the results from the several stations arises from the difference in the hours of observation, or represents an accidental feature of the years selected, it is impossible to say. The general conclusion is that of the three elements, potential gradient, conductivity and vertical current; the last-mentioned is the least variable.

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[*] Studie uber Licht und Luft des Hochgebirges, Vieweg & Sohn (1911).

[†] Meteorologische Zeitschrift, Vol. 47, p. 72.

[‡] Metl. Office, Geophysical Memoirs, No. 7.

[§] Bol. del Observatorio del Ebro, Vols. 9 to 14.

[||] loc. cit. Tab. 2, p. 346.

[¶] Met. Zeit., Vol. 41, p. 72.

§ 5.

In Table II. the Potsdam data correspond to the (A) series in Table I., and the Davos data are also for the same epoch as in that Table. At these two stations the entry under an hour really applies to 60 minutes *ending* at that hour. The Kew and Eskdalemuir results are from eye readings taken in 1914 by E. H. Nichols* and other members of the observatory staffs whose co-operation he secured. At Kew there were in all 12 days of observation, four from each month specified. For Eskdalemuir only six or seven days were available.

The potential gradient at Potsdam and Kew—and the same is true of many other stations—has a double daily oscillation, with minima in the early morning and afternoon, the latter increasing in prominence in summer. At Eskdalemuir, in summer, the morning minimum is hardly represented, and the fall to the afternoon minimum begins exceptionally early.

At the two British stations the conductivity and the potential gradient clearly vary in opposite directions, and the change in i is relatively small, but the days of observation were too few to secure smooth results. At Potsdam the current appears as variable as the conductivity, but the variations in both are comparatively small. The results for Davos in Table II. are for λ_1 only, but the total conductivity λ has a very similar diurnal variation. For the year as a whole, λ is much higher by night than by day, and according to Kahler† it is on the average about thrice as large as at Potsdam. Davos is about 1,550 metres above sea level. It would obviously be of interest to know whether it is fairly representative of high-level stations. The diurnal variations of potential gradient and air earth current, as well as conductivity, for the year as a whole at Davos are shown diagrammatically in a recent work by Dorno.‡ The potential gradient has a double oscillation, the forenoon minimum being much the more prominent. The air-earth current does not vary much between 6 p.m. and 6 a.m., but it is much reduced for some hours before and after noon.

§ 6.

Recently Dr. S. J. Mauchly,§ a member of Dr. Bauer's staff, claims to have established that "the diurnal variation of the potential gradient over the oceans is primarily due to a 24-hour 'wave,' which progresses approximately according to universal rather than local time" (l.c. p. 80), and further "there appear to be good grounds for concluding that in general the 24-hour wave of the potential gradient progresses approximately according to universal time over the entire surface of the earth."

The regular diurnal variation, it may be explained, can always be analysed into a series of Fourier waves, with periods of 24, 12, 8, &c., hours. For the land, Dr. Mauchly's conclusion is limited apparently to the 24-hour wave. At some stations this is the principal wave ; at others, e.g., Kew, it is not.

Dr. Mauchly's conclusions, if confirmed, would obviously be of great importance, as the greater part of the earth's surface is oceanic. The subject has been discussed

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* Phil. Mag., Vol. 32, p. 282 (1916).

† Meteorologische Zeitschrift, Vol. 47, p. 75.

‡ Klimatologie im Dienste der Medizin, p. 19 (1920).

§ Terrestrial Magnetism, Vol. 28, p. 61.

TABLE II.—*Diurnal Variation of Electrical Elements (Percentages of Mean).*

Hour.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
Potsdam, year, P	94	90	89	84	85	90	100	112	110	110	102	94	91	88	89	92	95	98	106	115	120	119	113	108
,, ,, λ............	106	111	113	113	113	109	101	91	87	87	93	97	99	100	101	100	101	101	99	96	95	94	95	95
,, ,, i............	101	101	101	97	97	101	101	101	101	97	93	93	89	89	89	89	93	97	105	114	118	114	109	109
Davos, February, λ_1	144	157	171	182	178	146	100	73	60	45	35	34	43	56	70	83	88	93	90	94	101	106	118	142
,, June, λ_1	140	155	161	169	156	109	68	56	64	70	69	72	74	79	84	90	94	90	86	89	95	104	109	117
Kew, summer, P............							112	120	117	109	102	100	84	75	79	78	85	103	111	123				
,, ,, λ							80	78	84	99	96	99	114	119	119	115	113	107	92	86				
,, ,, i							89	96	98	112	101	101	97	93	93	105	102	107	99	107				
Eskdalemuir, August, P				120	158	163	162	115	86	72	72	72	67	62	62	62	62	77	134	153				
,, ,, λ...				77	68	72	81	79	118	141	123	120	129	127	107	108	110	83	83	74				
,, ,, i...				92	95	110	122	97	111	124	112	101	103	96	99	71	78	72	117	97				

in recent Papers by Prof. G. Angenheister* and Mr. F. J. W. Whipple.† Both refer to the support which Dr. Mauchly's conclusions seem to derive from potential gradient observations in high latitudes. One objection, which had occurred also to myself, is that at Samoa observatory—of which Angenheister was long director—the diurnal variation of potential gradient near the ground appears to be of the ordinary double oscillation type at land stations; whereas in a small island in the Pacific one would *a priori* have expected oceanic conditions to prevail. Angenheister points out, however, that this double oscillation at Samoa seems confined to air very near the ground. At 8 metres the 24-hour wave prevails, but its phase does not accord with that of Mauchly's universal wave. Another circumstance which should be mentioned is that when the earlier portion of the material at Dr. Mauchly's disposal was discussed by Dr. Bauer and Prof. Swann‡ in 1917, the conclusion reached was that the diurnal variation at sea was similar to that at Potsdam and Kew. According to Dr. Mauchly (l.c., p. 61) "the difference between the conclusions of 1917 and those of 1921 is due chiefly to a difference in the method of reduction." It is obvious, of course, that the diurnal variation at a particular spot might consist of two parts, one purely local, the other universal with its maximum phase answering to a fixed hour G.M.T. If results were obtained from a number of stations, set out at equal intervals along a parallel of latitude, and if the part of the diurnal variation dependent on local time had everywhere the same amplitude, then if the hourly values at all the stations were set out according to Greenwich time and combined, there would result a diurnal variation corresponding to the part depending on universal time.

It is this universal part of the diurnal variation which Dr. Mauchly has apparently set out to isolate in the case of the ocean observations. A number of points are obviously involved, on which it would seem difficult if not impossible to form a judgment without a personal study of the material.

As a circumstance favourable to Dr. Mauchly's conclusions, Prof. Angenheister§ refers to a result which I reached when discussing the magnetic observations taken in 1911 and 1912 in the Antarctic at Cape Evans.|| It was then pointed out that the hours of maximum disturbance in the Antarctic appeared fairly similar to those at Eskdalemuir, when both were reckoned in Greenwich time, though local time at the two places differed by some 11 hours. This suggested the possibility that magnetic disturbance all over the earth might proceed according to universal time. If this should prove to be the case, it would, of course, reduce the mistrust with which most people welcome what seems to be an unique phenomenon.

It is perhaps only proper to remark that since discussing the Cape Evans data, I have studied the results obtained by the Australian Antarctic expedition in 1912-13 at Cape Denison. Treating the Cape Denison data in the same way as those at Cape Evans, I again obtain a conspicuous maximum of disturbance in what is the night at Greenwich, but the day at Cape Denison. This maximum, however, seems to be several hours later at Cape Denison—which is to the west of Cape Evans—than it is at Cape Evans.

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[*] Ges. d. Wiss. Nachrichten, Gottingen. Math.-Phys., Klasse, Heft 2, p. 81 (1924).

[†] Meteorological Magazine, Vol. 59, p. 201.

[‡] Department of Terrestrial Magnetism, Carnegie Institution. Researches, Vol. 3, p. 422 (1917).

[§] loc. cit., p. 98.

[||] British Antarctic Expedition, 1910-1913, Terrestrial Magnetism, p. 144.

§7.

To render clear the position as to longer period variations, a brief reference is necessary to terrestrial magnetism. As is well known, the amplitude of the regular diurnal magnetic changes shows a large variation from year to year, at least approximately parallel to the variation in sunspot areas or frequencies. There is also, at least in high latitudes, an increase in the amplitude of the regular diurnal changes on days which are magnetically disturbed. The only reasonable explanation hitherto proposed is an increase in the electrical currents in the upper atmosphere, to which the regular diurnal changes are now generally ascribed. The increased current amplitude is most naturally explained by increased ionization and conductivity. As severe magnetic storms are usually accompanied by Aurora, it is reasonable to suppose that the height of the Aurora indicates roughly the level of the highly ionized atmosphere. Some observers have claimed that they saw Aurora below the summits of mountains, and other phenomena consistent with this view have been described; but it has received no support from the recent exact measurements of Auroral heights made in Norway by Prof. Carl Störmer* and others. According to these the lower edge of Aurora is usually at a height of from 90 to 100 km. Thus, while magnetic phenomena support the existence of the sunspot period in atmospheric conductivity at heights of 90 km. and upwards, they seem to throw no direct light on its existence in atmospheric electricity in the lower atmosphere. Obviously, however, it would not be at all surprising if it did present itself there, and Dr. L. A. Bauer† claims that it is present and far from negligible in the data published at Tortosa for potential gradient. Kew‡ data are not inconsistent with the existence of a *small* 11-year variation, but they do not suggest any such considerable effect as Dr. Bauer described at Tortosa. There are, unfortunately, few stations having homogeneous data for even one sunspot cycle, so the question is unlikely to be settled definitely for some time to come.

§8.

There are a number of possible sources§ for the ionization of the atmosphere, including radioactive emanation in the air itself, γ-rays from the radium and thorium in the soil, photo-electric effects at the ground, ultra-violet light and penetrating radiation. Over the oceans and lakes ionization may be produced by the breaking of waves.

Direct radio-active effect from the soil will naturally be effective mainly if not exclusively in the lowest layers of the atmosphere; while the effects of ultra-violet light would naturally be much greatest in the highest layers. The radio-activity of the soil and the radio-active emanation in the air seem alone quite adequate to account for the ionization usually measured near the ground. Experiments with closed vessels have shown the existence of a penetrating radiation. The difference between the results obtained over land and water show that on land a substantial part of this comes from the ground. As we ascend, the penetrating radiation, according to the observations of Hess[*] and W. Kolhorster,[†] diminishes to a minimum at a height of from 500 to 700 metres, and then increases again. At 2,000 metres it is greater than at ground level and it increases notably at higher levels. Observations made recently by R. A. Millikan[‡] in America so far agree with Kolhorster's, but they make the increase with height much less. It has been suggested that part of the penetrating radiation may come from sources external to the atmosphere, but this view does not seem supported by Millikan's observations.

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* Terrestrial Magnetism, Vol. 21, p. 157, and numerous other Papers and memoirs.

† Terrestrial Magnetism, Vol. 27, p. 1.

‡ Proc. Phys. Soc., Vol. 35, p. 129.

§ c.f. Chauveau, Électricité Atmosphérique, Troisieme Fascicule, Chap. 6.

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[*] Physikalische Zeitschrift, Vol. 12, p. 998, &c.

[†] Physikalische Zeitschrift, Vol. 14, pp. 1066 and 1153, and Terrestrial Magnetism, Vol. 20, p. 82.

[‡] c.f. C. V. Ockenden, Meteorological Magazine, Vol. 59, p. 220.

GEOPHYSICAL INFLUENCES ON THE TRANSMISSION OF WIRELESS WAVES.

By Prof. E. V. APPLETON, *M.A.*, *D.Sc.*

ABSTRACT.

The facts of short and long-distance transmission for long and short waves are summarised and the examples of variable signals attributed to the varying phase and amplitude of rays ionically refracted by the upper atmosphere. The diurnal variation of such signals is described, and the strong night signals attributed to a lower atmosphere devoid of excessive ionization. Explanations of the difference between the magnitudes of directional errors for overland and over-sea transmission and of the marked success of nocturnal short-wave transmission are put forward.

The salient points of the evidence relating to the seasonal variations of signal strength and to the influence of the configuration and inhomogeneity of the earth's surface are also summarised.

The facts of wireless telegraphy, as set forth show that the atmosphere exerts a variable but usually favourable influence on wave propagation. This influence may be interpreted in terms of ionic refraction (Eccles) which, to prevent excessive dissipation, must take place at levels sufficiently high to make the mean free paths of the effective carriers large (Larmor).

THE various facts concerning the role of the atmosphere in Wireless Telegraphy have resulted from the comparison of the received signal intensities with the theoretical values worked out on the assumption of a perfectly conducting earth and a perfectly insulating atmosphere. It was very soon found that marked discrepancies between observed and calculated values were evident, especially when the transmission took place over large distances, which compelled a closer scrutiny of the physical assumptions underlying the mathematical investigations. The laws of electrodynamics are certainly valid for such low frequencies so that all recent work has been concerned with linking up the discrepancies with the finite resistance of the earth and the ionization in the atmosphere.

In approaching the somewhat complicated facts of signal transmission it is of great assistance to bear in mind certain broadly-defined distinctions. In the first place we must be careful to distinguish between transmission over distances small enough for the earth to be considered plane from transmission over larger distances where the curvature of the earth has to be taken into account and diffraction is all-important. Also we must recognize that the results for short waves (e.g., of the order of 300 metres) are usually quite different from the results obtained with long wave-lengths (e.g., of the order of 10,000 metres). Lastly, we must recognize that the results for day and night conditions are often very different, especially in the case of short waves.

Bearing these differences in mind I have dealt with the subject under two headings, (I) short distance transmission and (II) long distance transmission, and the results with long and short waves during day and night conditions are discussed under these headings. Other points which seem to require special mention are dealt with under headings (III) to (VI).

I. SHORT-DISTANCE TRANSMISSION.

If an alternating current of maximum amplitude I_0 and angular frequency ω passes through an element of length ds, the electric intensity E of the radiation field at a distant point in a plane at right angles to the element is given by

$$E=\frac{I_0\omega ds}{rV},$$

where r is the distance of the point from the element and V is the velocity of electromagnetic radiation in free space. If this formula is applied to the radiation field of a wireless aerial the value of ds to be inserted (known as the effective height) is the value which fits the equation when E is measured at a point sufficiently far from the origin to make the induction field negligible and yet sufficiently near for there to be no marked absorption of radiation. According to this formula the field intensity should vary inversely as the distance. Duddell and Taylor[*] nearly twenty years ago carried out the first accurate intensity measurements with a view to testing this relation for short waves over short distances. Over sea-water the inverse-distance relation was found to hold accurately up to 96 km. On the other hand, discrepances were found for overland transmission at distances as small as 1,800 metres. The signal strengths were less than the calculated values due to the absorption of the badly conducting ground. That the departure of the law of decay from the simple inverse distance law is due to the absorbing action of the ground is evidenced by the fact that signals can be obtained with a horizontal antenna pointing in the direction of propagation of the waves. The correlation of such a horizontal component of electric force with ground absorption was indicated by Epstein[†] and follows directly from an application of Poynting's theorem.

With longer waves the attenuation is by no means as marked over land. For example, Austin[‡] compared transmission over 83 km. for wave-lengths of 1,000 and 3,750 metres. In the former case the electric force was less than one-fifth of its calculated value while in the latter case the two agreed within the limits of experimental error. All other observations seem to show that the absorption is only marked for short waves, and that the longer the wave-length the more nearly is the inverse distance law of electric force attenuation obeyed.

The effect of ground absorption for short waves over short distances has been very thoroughly studied recently by Bown and Gillett[§] who studied the space variation of electric force round two American broadcasting stations. Their results are embodied in two most instructive diagrams given in their paper, one of which shows, as a graph, the relation between the decay of electric force and the topography of the district, while the other exhibits the field strength contours superimposed on an aerial photograph of the district. These diagrams illustrate in a most marked manner the difference in attenuation over land and over water. This difference may be expressed quantitatively, for, if we assume with Austin that the absorption may be allowed for by multiplying the above expression for the electric force by $e^{-\frac{\alpha r}{\sqrt{\lambda}}}$, where λ is the wave-length, the value of α for dry and sandy ground is 0·28, while for over water transmission it is only 0·0025.

The curves further illustrate the marked shielding influence of mountains, first noticed by Sir Henry Jackson,[||] at points on the immediately far side of the obstacle. As the obstacle was of the same order of dimensions as the wave-length (469 metres) the shielding effect disappeared at greater distances.

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[*] Journ. I.E.E., Vol. 35, p. 321 (1905).

[†] Jahrbuch der drahtlosen Telegraphie und Telephonie, Vol. 4, p. 176 (1910).

[‡] Bulletin of Bureau of Standards, Vol. 7 (1911).

[§] Proc. I.R.E., Jan. 16 (1924), Figs. 3 and 7 in particular.

[||] Proc. Roy. Soc., May 15 (1902).

The above observations were made during the daylight hours when steady and consistent values can usually be obtained. At night there is a marked difference in that for transmission distances over 50 miles the intensity is variable. These variations in general are more marked the shorter the wave-length, though there is a certain amount of evidence to show that at very short wave-lengths (e.g., 50-100 metres) the variations are less than on higher wave-lengths (e.g., 200-500 metres).

A good deal of information relating to night-time variation of short wave-signal intensity has been published from time to time, mainly by wireless amateurs, but as the results are expressed in terms of telephone audibilities, the measurements are of little scientific value. The only accurate measurements of short-wave intensity variation that I am aware of are those carried out by Pickard in America, and by wireless students* in Cambridge. In these experiments a galvanometer has been used as the recording instrument. The results show that although there is very little variation of intensity during the day, most marked variations occur beginning round about sunset. The effect is very marked at distances of 100 miles, but even at Cambridge, which is only 55 miles from London, the night variations of the waves from the London Broadcasting Station can be detected and measured. The relative magnitude of the changes depends on the relative magnitudes of the ground or direct ray, and the reflected or indirect ray from the upper atmosphere. During the day the indirect ray due to its refraction in the lower layers of the atmosphere is very weak, and the signal is to be attributed mainly to the direct ray. Taking the Cambridge measurements we find that with a receiving set of a certain sensitivity the signal from London is large compared with that from Bournemouth, which is barely measurable. After sunset the variations of the London signal are relatively small, the daytime mean value being practically maintained. On the other hand the Bournemouth signals increase and decrease by relatively large amplitudes, the mean night value being much bigger than the mean day value. It seems difficult to account for these results in any other way than by assuming that the intensity of the indirect ray at night in the case of London is small compared with the direct ray, and that the night effect is to be attributed to interference between the two rays, the varying phase and amplitude of the indirect ray being the determining factors. In the case of Bournemouth signals, however, the indirect ray is much stronger than the direct one, and so the large observed changes are mainly due to the intensity fluctuations of the former.

For distances sufficiently large for the effect of the reflected or indirect ray to be appreciable, marked variations of bearing as measured by coil direction finders are noticed. These variations were first described by Hoyt-Taylor and Eckersley, and were considered by them to be an effect of the upper atmosphere. If we attribute these errors to the interference of the two rays and assume that the reflected one is the variable one, we should expect the errors to be noticeable at shorter distances over land, where the direct ray is strongly attenuated, than over sea, where little absorption takes place. This is actually found to be the case. Directional errors are noticed for transmission over land at distances as small as 30 miles, while over sea errors are rarely significant until the path is 80 miles.

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* In particular I should like to mention Mr. M. Barnett and Mr. F. G. G. Davy.

II. LONG-DISTANCE TRANSMISSION.

Here there is a discrepancy between the theoretical and experimental values for both long and short waves. These differences are found not to be attributable to the finite conductivity of the earth, and the discrepancy is usually put down to the ionization in the atmosphere.

The case of short waves will be considered first. Here the most accurate experimental results are those obtained by the engineers of the Western Electric Company* who, in connection with tests preparatory to the inauguration of a Trans-Atlantic telephone service, investigated the transmission of short waves over sea and over distances for which there is an appreciable curvature of the earth's surface. During the day the values of signal intensity for distances up to 1,100 miles were found to be closely in accordance with the values given by the empirical formula of Austin, which is simply the ordinary radiation formula for a flat earth multiplied by an attenuation factor of the form $e^{-\frac{ar}{\sqrt{\lambda}}}$, where a is 0·0015, when r and λ are in kilometres.

At night much higher values of signal intensity were obtained which agree approximately with the theoretical values if the attenuation factor is neglected. This has led some American engineers to say that the old difficulty of accounting for the large observed intensities for large distances does not exist, but that the atmosphere at night can be regarded as insulating, and a simple inverse distance law is to be expected to hold. Such an interpretation is, of course, in opposition to the results of the mathematicians who say that diffraction alone, especially with short waves, cannot possibly account for the observed intensities. Most probably the explanation is that the reflection of the main indirect ray is almost complete and as its linear path for long-distance transmission is not so different from the direct ray path, the inverse distance law holds approximately.

With longer waves the Austin formula gives fairly accurate prediction, but here the difference between day and night intensities are by no means as marked.

Fuller† in 1914 experimented between Honolulu and San Francisco, over a distance of 3,880 kilometres, the transmission being almost all over sea. The length of wave was increased in steps from 3,000 to 11,800 metres. During the day the received signal increased steadily with increase of wave-length, but during the night the intensity wave-length curve showed maxima and minima. We may interpret this as indicating that during the day the indirect rays are feeble, so that increase of wave-length in increasing the effect of pure diffraction brings about an increase of signal strength, while, at night, when the direct and indirect rays are of the same order of magnitude, interference bands, which alter with the wave-length of the radiation, are produced.

For long-distance transmission the day-time signals are usually much larger than those to be expected from a pure diffraction theory, so that favourable atmospheric influences are indicated even in the day-time. It may further be mentioned that this influence is not always still more favourable at night in the case of very long waves, for the mean day and night signals for wave-lengths of the order of

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* **Proc. I.R.E., June (1923).**

† **Electrician, May 7 (1914).**

10,000 metres are not very different. The marked difference between day and night signals is to be found only with the shorter waves.

If we are to attribute the excess of signal strength over the theoretical value predicted by diffraction theory to the effect of an ionized layer, the difficulty has been raised that the sun's radiation should produce such a layer in the day time and we ought to expect the effect of the ionized layer to be most marked during the day. The answer to this difficulty seems to be as follows: To produce sufficient refraction to bring an inclined ray horizontal we require a certain increase of phase-velocity at some place midway between the two stations. Due to atmospheric ionization caused by the sun's radiation, this particular increase of phase-velocity is reached during the day at levels at which the pressure is sufficient to make the collisional "friction" experienced by the ions strongly dissipative, and the ray is absorbed. At night, when the atmosphere is cleared, this particular increase of phase-velocity is reached at higher levels, in which the ray can be turned but not absorbed. In the latter case, as Larmor has recently shown, it is not necessary to assume large ionic contentrations. In such a case it is correct to speak about a *conducting* layer, since there is less "friction" there than at the lower levels, but the term *ionized* layer may not always apply, for in some cases it does not seem necessary to consider the effective layer as more ionized than any other layer in the atmosphere.

III. DIURNAL VARIATION OF SIGNAL STRENGTH.

Various facts concerning this variation have been mentioned in the general sections relating to long and short waves. When the distance of transmission is not large the direct ray is the principal factor in reception, and practically no variations are noticed. For greater distances the marked but sometimes variable increase of signal strength is the most important phenomenon, especially with short waves. For transmission in an east and west direction curious changes are noticed during the periods when a sunset or sunrise band is between the transmitting and receiving stations. Signals are then abnormally low. The sunset band seems a bigger obstacle to electric wave transmission than the sunrise band, for the received signals are almost consistently lower during the period between the two local sunsets than between the two local sunrises. During the time when the sunrise band is between the two stations a temporary maximum is often noticed, so that two sunrise minima are obtained. One marked minimum seems to be the effect of the sunset period.

The effect of a solar eclipse has been in almost all cases a partial or complete return to night-time conditions, an increase of signal strength being the rule.

IV. SEASONAL VARIATION OF SIGNAL STRENGTH.

Very few observations on the nature of this variation have been made, but the general result of the available evidence is that for long-distance transmission the signals are stronger during the winter than during the summer, but that the relative amount of winter increase seems to vary from year to year. It is usually found that the annual variation is more marked for night transmission than for transmission during the daylight hours.

V. EFFECT OF DIRECTION OF TRANSMISSION.

Some observers have noted that it is easier to signal in a north and south direction than in a direction at right angles. Whether this is due to the influence of the earth's magnetic field or is merely due to the absence of sunrise and sunset bands in the former case remains to be decided.

It seems fairly well established that directional variations as indicated by a loop receiver are less troublesome for north-south transmissions than for the direction at right angles.

VI. INFLUENCE OF CONFIGURATION AND INHOMOGENEITY OF THE EARTH'S SURFACE.

The shielding influence of obstacles (e.g., mountains and trees) was first described in detail by Sir Henry Jackson ; such effects are, as we should expect, the more marked the shorter the wave-length. Quantitative values of signal strength relating to the shielding effect of mountains have, as previously mentioned, been given by Bown and Gillett.

The different conductivities and dielectric constants of sea-water and land lead to different velocities of propagation of waves along them, and Eckersley* has shown how this accounts for the refraction at coast lines. The velocity of the waves over sea-water is greater than over land, resulting in an effective refractive index of the order of 1·02. Similar effects were found by Bown and Gillett in America. The importance of refraction of this kind diminishes when the length of the wave increases.

APPENDIX I.

In the above account of signal variations I have, for conciseness, used the word "reflection" with reference to the ray returned from the upper atmosphere. It is, of course, unlikely that there is a boundary between a non-conducting layer and a conducting one, the transitional element of which is, in dimensions, comparable with a wave-length of the radiation. The ray must be returned by a kind of ionic refraction due to a phase-velocity increased by the presence of free ions of large mobility. The theory of metallic media here applies, as suggested first by Eccles,† but it has been shown recently by Larmor‡ that the kind of conditions contemplated by Eccles involve so much absorption that the effect on the received wave would be almost negligible. The way out of the difficulty seems to be to assume, with Larmor, that the part of the atmosphere which is effective is that in which the electrons or ions experience very little collisional "friction." The question remains to be decided as to whether electrons or molecular ions are effective, and the question may be decided, I think, by considering the action of the earth's magnetic field in connection with wave transmission. If we consider transmission along the earth's magnetic field H_0 in a medium containing electrons or ions we find that the phase velocity v becomes (in Heaviside units)

$$v = \frac{c}{\left(1 - \dfrac{Ne^2}{\omega^2 m \pm \dfrac{eH_0\omega}{c}}\right)^{\frac{1}{2}}}$$

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* **Radio Review, Vol. I, June (1920).**
† **Proc. Roy. Soc., Vol. 87 (1912).**
‡ **Nature, November 1 (1924).**

where c is the velocity of light, N the number of charged particles of mass m and charge e per c.c., and ω the angular frequency of the radiation. We thus see that a linearly polarized ray in such a case would be split up into two circularly polarized rays travelling with different velocity, and the resultant plane of polarization would be rotated in the usual way. Such a rotation may be expected to have an effect on the observed direction of the waves as indicated by a direction-finder. But the chief interest of the formula seems to be that for a wave-length of about 580 metres and a magnetic field H_0 of the order of 0·18 gauss, the terms $\omega^2 m$ and $\frac{eH_0\omega}{c}$ are of the same order of magnitude for electrons, in which case a kind of resonance would take place for one of these rays. This frequency corresponds to the frequency with which the free electrons circle round the lines of magnetic force, due to the velocity of thermal agitation. Thus peculiar things might be expected to happen for wave-lengths round about this value if the carriers are electrons. On the other hand if the carriers are molecular ions, this critical wave-length would be of the order of 10^8 metres, and so would be completely outside the wireless spectrum, except so far as atmospherics are concerned.

On general grounds we should expect the lack of absorption due to the large electronic or ionic free paths to be more marked the higher the frequency, for at high frequencies the electron or ion would make many oscillations between the time of two collisions. Possibly this is the explanation of the remarkable success of amateur transmissions with low power and short waves at night.[*]

APPENDIX II.

I have consulted one or two of the amateur experimenters who have met with such marked success in sending signals over large distances with low power and short wave-lengths. I find that the exact value of the series capacity in the aerial seems to be all-important, in which case we may infer that the mode of excitation of the aerial has a good deal to do with their efficient transmission. It seems most probable that the aerial must be excited at a frequency higher than its fundamental, in which case, for resonance periodicities, as shown by Van der Pol,[†] the direction in which maximum energy is radiated is not along the ground, but in a direction inclined to it. We thus arrive at the somewhat remarkable conclusion that to prevent marked attenuation we need only keep the wireless ray off the ground; in other words, for efficient transmission at night, we must take special steps to send the maximum amount of energy into the upper atmosphere.

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[*] If this suggestion is correct we might expect that the reduction of the wave-length to very low values would make daylight transmission of small attenuation possible in the lower layers of the atmosphere which are ionized by solar radiation; for a tenfold increase of frequency and a tenfold reduction in the time between two ionic collisions (due to a reduction in the height of the effective atmospheric layer) would leave the co-efficient of absorption unaltered.

[†] Proc. Phys. Soc., June 15 (1917). In this Paper Van de Pol actually suggests that a loaded antenna of this kind might be used for detecting the ionic refraction in the upper atmosphere.

ATMOSPHERICS.

By R. A. Watson Watt.

Abstract.

The Paper presents a brief review of data, as to the nature and origin of atmospherics obtained in the course of a fundamental investigation of atmospherics undertaken by the Radio Research Board. The determination of the wave form of atmospherics by cathode ray oscillograph is referred to, and typical constants of peak field strength and duration are quoted. The results of an examination of the meteorological conditions prevailing at apparent sources of atmospherics, as located by direction finding, are tabulated, and show that even with the limited data available relations with thunderstorm phenomena, precipitation, or barometric minima have been traced, in 87 per cent. of 490 cases, distributed over Europe, the Mediterranean, and North Africa. The tracing of a "cold front" for 40 hours and 2,000 k. across Western Europe, by directional recording of atmospherics, is reported. Reference is made to the fine structure of atmospherics in relation to their interferent properties. The present state of the investigation is summarised.

I. Introduction.

THE inclusion in a discussion on "Ionization in the Atmosphere" of these notes on naturally occurring electromagnetic waves of relatively low frequency may, like the short title "Atmospherics" now generally applied to such waves, be suspected of begging the question of whether atmospherics are in fact atmospheric. But with the reservation that in the present state of our knowledge we cannot determine whether all atmospherics are products of ionization in the terrestrial atmosphere, it may be asserted that a very large proportion of atmospherics are such products, that there is no substantial evidence of any other origin for atmospherics, and that the known facts as to atmospheric processes are compatible with the hypothesis that the known supply of atmospherics, great as it is, results from ionization and convection in the atmosphere.

The atmospheric, in common with the most prominent of the meteorological elements, has a dual aspect, first as a geophysical phenomenon, and second as an interfering agent in human activities, and as in the case of its meteorological fellows its interferent effects have compelled attention to its geophysical origins. It is, accordingly, the purpose of this note to review very briefly some of the evidence which has been obtained in recent years as to the nature and origin of atmospherics, and to refer to some new data explaining their formidable powers of interference in radiotelegraphy.

II. Wave Form of Atmospherics.

Reversing the chronological order in which the data were obtained, a summary of recent determinations of the nature of atmospherics may be given. In a Paper by Prof. Appleton and the writer* will be found a description of the method by which the temporal variation of electric field constituting an atmospheric, and which may be briefly called the "wave-form" of the atmospheric, has been delineated. The method consisted in coupling a simple aerial circuit of known constants, by way of a coupling condenser, to a sensibly distortionless triode amplifier, whose output was applied to a sensitive cathode ray oscillograph. The screen image produced by the simultaneous application of the amplifier output voltage and of a locally generated and controlled voltage, providing a time base, was delineated by hand.

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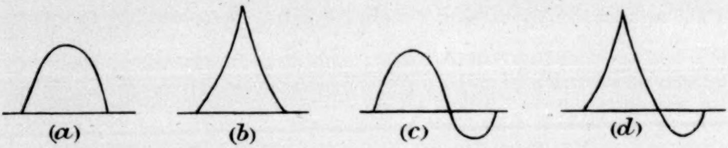
* **Watt and Appleton, "The Nature of Atmospherics—I," Proc. Roy. Soc., A, 103, 84 (1923).**

Since the publication of that Paper the collaboration has continued, with the assistance of Mr. J. F. Herd, and has been supplemented, through the courtesy of the Admiralty, the War Office, the Air Ministry, the Physical Department of Egypt, and the Sudan Government, by the delineation of typical wave-forms of atmospherics in the Middle East. As a result of this work, there are at present in process of reduction, or awaiting reduction, more than fifty thousand drawings of atmospherics observed in south-east England, the Mediterranean, Red Sea and Indian Ocean, in the vicinity of Cairo, and at Khartoum.

It is, however, sufficient for the purpose of the present discussion to outline the main characteristics of atmospherics observed in winter in south-east England. From the first six hundred samples analysed it was found* that the average atmospheric produced a maximum field change of the order of one-eighth of a volt per metre, and that the disturbance from the level field strength persisted for periods of the order of one or two thousandths of a second. Two main types occurred almost equally frequently: in one the disturbance consisted of a departure to one side of the level field, followed by a departure in the opposite side—i.e., a quasi-periodic disturbance consisting of one complete oscillation, whose mean duration was of the order of 2,000 micro-seconds, and in which the maximum electromotive force produced was most frequently directed down the aerial. The two main sub-types of this class are represented in (*c*) and (*d*) of Fig. 1. In one instance four complete oscillations were observed in a disturbance of this type. The second main type consisted of an aperiodic disturbance, most frequently with a duration of some 1,250 micro-seconds, and with a peak field strength change of the same order as in the quasi-periodic type. The main sub-types of aperiodic disturbance are represented in (*a*) and (*b*) of Fig. 1.



Typical Atmospheric Waveforms.

FIG. 1.

The wide departure of these forms from the simple exponential type which had customarily been assumed in theoretical work is of considerable interest. These early observations were made with a sinusoidal time-base, with which it was difficult to discriminate the order of temporal incidence of the various portions of an atmospheric, and all subsequent observations have been made with a linear unidirectional time base developed for this work by Appleton, Herd and Watt. This discriminating time base makes possible a fuller classification of sub-types of atmospheric; the data now being prepared for publication are being classified according to the order of incidence of steep and gentle slopes, and of the varieties of half-cycle. No profound modification of the fundamental values discussed is likely to arise from the later English observations.

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* **Watt and Appleton, loc. cit.**

Many of the fundamental wave-forms observed were found to have a fine structure, in which the elements were comparable in duration with the lower commercial radiotelegraphic frequencies; attention was drawn to this fact in the Paper under reference, and a further note on this structure will be found later in the present Paper.

III. Extreme Values.

The extremes of field-strength, duration and numerical frequency experienced in the course of the observations are, naturally, very diverse. The number of atmospherics per unit time passing a selected peak field strength increases very steeply as the selected field strength decreases. At the upper end of the scale of radiation fields definitely measured stands an atmospheric observed at Khartoum, which produced a voltage of 250 in the aerial, corresponding to 4·2 volts per metre. As to the lower limit of duration one can say little more than that the present limits of resolving power of the combination of cathode-ray oscillograph and observer reveal many apparently isolated peaks of the order of 100 micro-seconds. The average lightning flash would appear to produce a series of detached complex discharges, in which some three to six discharges, of duration about 0·003 sec., are separated by quiet intervals of 0·002 sec. to 0·003 sec., giving a total disturbed period of the order of 0·01 sec. to 0·03 sec. In one instance, however, near Salonica, in June, 1924, the writer has observed many lightning discharges in which there was no sensible interruption of the visible illumination, and sensible continuity of the audible disturbance in a radio-receiver, over periods of four and five seconds.

Simultaneous measurements have also been made, abroad and in south-east England, of the number of atmospherics per unit time whose peaks exceeded specified limits, but the results await reduction.

In relation to the adequacy of the processes of atmospheric ionization to account for the whole supply of atmospherics, it is very necessary that a reliable estimate of the world supply of lightning may be made and published. As a very slight contribution to the estimate, the writer may record that in his brief experience of the Tropics he observed, on at least three occasions (one in November on the African coast of the Red Sea, two in April at Khartoum), groups of storms in which flashes occurred at a mean rate of one per second throughout a period of three hours. Doubtless higher flash frequencies are experienced in the true thunderstorm seasons in those latitudes.

IV. The Atmospheric as a Meteorological Phenomenon.

Turning to the atmospheric as a meteorological phenomenon, no more than a passing reference is now required to the early work on the meteorological relations of atmospherics which has been summarised in a Paper by Mr. C. J. P. Cave and the writer.*

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* Cave and Watt, "Study of Radiotelegraphic Atmospherics in Relation to Meteorology." Q.J. Roy. Met. Soc., 49, 35 (1923).

It is now possible, however, to give revised data as to the relation between apparent sources of atmospherics, on the one hand, and thunderstorms and other meteorological phenomena on the other, which formed the subject of a preliminary note in " Nature."*

In the investigation of this relation the results of observations on the direction of arrival of strong atmospherics at British stations were employed, the apparent source of the predominant atmospherics at certain times being found by the intersection of bearings from three or more stations. These locations of apparent sources were compared with meteorological data for the time and place concerned, much unpublished thunderstorm data having been supplied (since the date of the note in " Nature ") by foreign meteorological Institutes through the courtesy of the Director of the Meteorological Office, Air Ministry. It is to be regretted that time has not yet been found for a really exhaustive study of the non-thunderstorm data, the method adopted has been to determine the place and time of occurrence of the nearest reported thunderstorm, and, in the absence of thunderstorms, to search for records of hail, passing showers, squalls and rain, and to examine the barometric distribution. In order to present a brief summary of the result Table I. has been prepared, showing the number of apparent sources of atmospherics located in various regions between April, 1916, and April, 1918, with the percentage number of such locations which have been identified as associated with

(*a*) Thunderstorm phenomena within 250 kilometres.
(*b*) ,, ,, 1000 ,,
(*c*) Hail showers within some 200 kilometres.
(*d*) " Passing showers " within a similar distance.
(*e*) Squalls.
(*f*) Rainfall.

The allotment of any individual case to one column implies the absence of reported phenomena belonging to earlier columns—otherwise expresed, the cases are alloted to the highest degree of " thunderiness " reported. The inadequacy of the criteria is clear; hail, passing showers and squalls have not yet been limited to meaning the same things for all observers; and all the criteria adopted, save lightning, are merely collateral effects, and not causes, of the phenomenon of electromagnetic radiation. But in the default of a closer study of the physical processes the classification may serve. It is supplemented by a column in which, when no thunderstorm, squall or precipitation phenomena have been traced, the occurrence of barometric minima, troughs of secondary and V-shaped depressions, and marked local irregularities of isobars near the time and place of apparent origin of atmospherics, has been noted. The re-study of the whole of the data on the lines of this column, or preferably with reference to the " cold fronts " and " warm fronts " with which isobaric irregularities are associated, would be interesting. Meanwhile, the fact that a meteorological correlation of sorts has been found in all save 3 per cent. of the British cases, and, with very inadequate data, in 87 per cent. of all cases is

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* **Watson Watt, " The Origin of Atmospherics," Nature, 110, 680 (1922).**

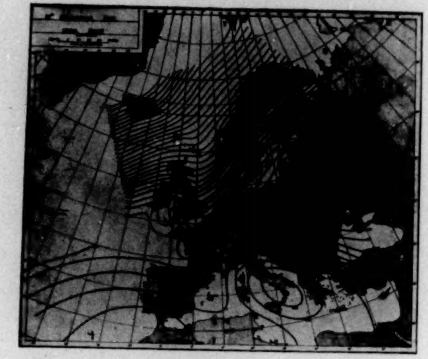

FIG. 2A.

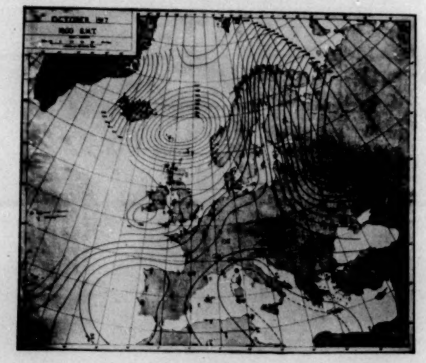
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FIG. 2B.

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significant, especially in view of the comparatively large number of locations, averaging two per three days during the period.

TABLE I.—*Meteorological Conditions near Apparent Sources of Atmospherics.*

Region.	No. of locations	Thunderstorms within 250 k. %		Hail %	Passing showers %	Squalls %	Rain %	Isobaric minima &c. %	No correlation found. %
			within 1,000 k. %						
England & Wales	58	35	17	5	14	14	7	6	2
Ireland	45	13	13	2	44	16	8	0	4
Scotland	17	0	29	18	35	6	6	0	6
British Isles*	120	22	18	6	28	13	7	3	3
English Channel...	58	31	7	2	20	9	15	7	9
France	146	36	4	2	1	1	27	13	16
Spain	23	13	—	—	—	—	52	13	22
Italy	19	42	—	—	—	—	26	21	11
Holland	7	86	14	—	—	—	—	—	0
S.W. Europe*......	373	30	9	3	13	6	20	9	10
Central and S.E. Europe	12	33	8	—	—	—	—	34	25
Bay of Biscay ...	38	—	11	3	—	—	60	3	24
Iceland and Atlantic	10	—	10	—	—	—	10	50	30
Belgium	7	29	43	—	—	—	—	—	29
Switzerland	6	33	—	—	—	—	—	17	50
Mediterranean and N. Africa	44	2	27	—	—	—	11	46	14
TOTAL	490	25	11	2	10	5	21	13	13

The mean distance between an apparent source and the corresponding "near" reported thunderstorm is approximately 100 kilometres, the corresponding value for "distant" thunderstorms is between 300 and 400 kilometres. Distances over 500 kilometres are rarely used save in regions where data is insufficient—e.g., Italian thunderstorms have had to be used in considering North African sources. It should be noted that the distance between apparent source and reported storm is not to be regarded as a measure of error in location, the storm is merely the nearest reported index of conditions in the vicinity of the source. In 21 cases the location fell within 20 kilometres, and in 44 within 50 kilometres of a reported thunderstorm.

Of special interest are some of the cases in which the apparent source of atmospherics lies in a barometric minimum from which there is a definite report of good weather without thunder or precipitation. The synoptic charts for two such cases are shown in Figs. 2A and 2B.

A further line to be prosecuted in regard to these data, and in relation to thunderstorm data, is the explanation of the general diurnal and seasonal variations in apparent direction of arrival of atmospherics. Fig. 3 shows the distribution of

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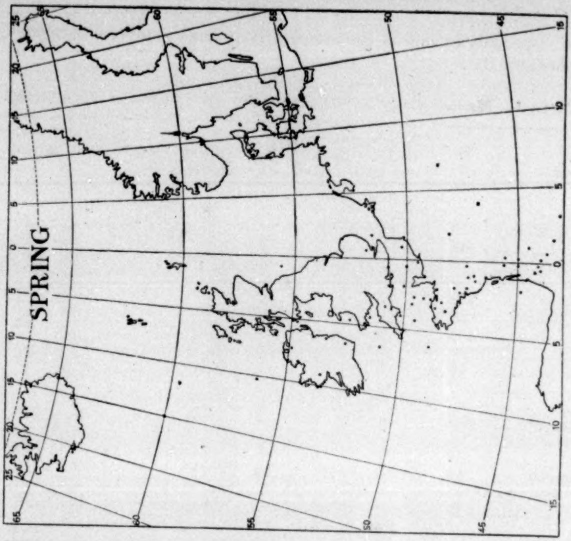
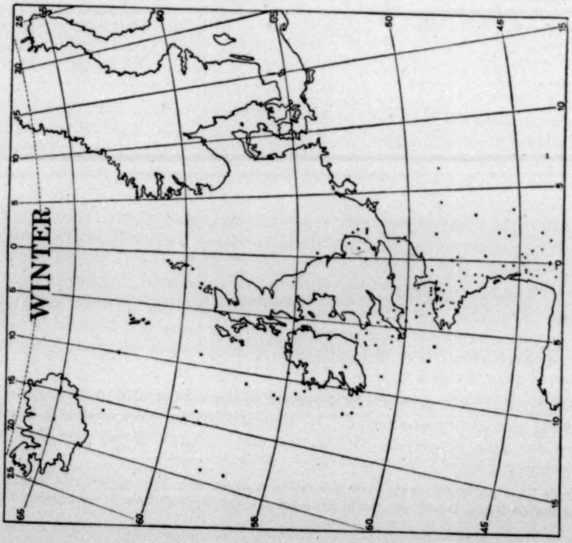
* These lines represent sums from preceding regions, the three apparently arbitrary groups thus constituted being based on consultation of meteorological data of decreasing adequacy.


SPRING
65
60
55
50
45
25
20
15
10
5
0
5
10
15
20
25


WINTER
65
60
55
50
45
25
20
15
10
5
0
5
10
15
20
25

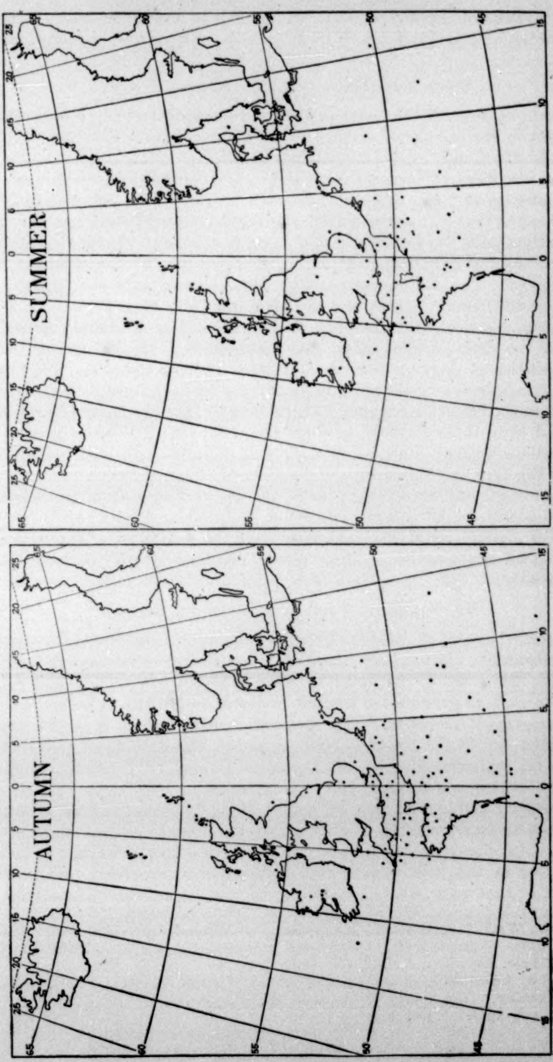

SUMMER
AUTUMN
65
60
55
50
45
25
20
15
10
5
0
5
10
15

FIG. 3.—SEASONAL DISTRIBUTION OF APPARENT SOURCES OF ATMOSPHERICS.

apparent sources in the four seasons, which may be compared with the published directional data,* and which has in itself some features of meteorological interest.

V. A COLD FRONT TRACED ACROSS EUROPE.

A recent example of the manner in which the European meteorological situation may dominate the supply of atmospherics in these Islands has been investigated by Mr. J. F. Herd, and by the Meteorological Office.

The Radio Research Board has for some years maintained a directional recorder for atmospherics at their Aldershot Station. In July of the present year a second recorder built at that station was installed at the Geophysical Observatory at Lerwick. After two days' trial run the recorder began routine work at 10 a.m. on July 12, 1924. The meteorological situation at 7 a.m. on that day was as shown on the synoptic chart, Fig. 4A. The well-marked trough then situated over the Atlantic can be followed on the synoptic charts for the 13th and 14th, Figs. 4B and 4C, and more clearly on the chart of isochrones of trough position, Fig. 5, kindly supplied by the Director of the Meteorological Office. At 2 p.m. on the 12th, the directions of arrival of atmospherics at the two stations intersected off the Hebrides. The apparent source of atmospherics was traced hour by hour by similar intersections, the trajectory is plotted on the isochronous chart. It will be seen that the sources of atmospherics are located close to the cold front which lies along the barometric trough, and are in very satisfactory agreement with reports of thunderstorms on the front between 1 p.m. on the 12th at Stornoway, and 1 a.m. on the 14th at Posen.

It is of incidental interest that during the period of six dark hours, shown dotted on the trajectory, no minimum direction of disturbance (from which is inferred the direction of arrival) could be determined from the record for either station—both records ceased simultaneously to be useful directionally, and resumed usefulness simultaneously.

VI. THE FINE STRUCTURE OF ATMOSPHERICS.

Reference has already been made to the interferent properties of the atmospheric in radiotelegraphy. It has been remarked† that the relatively smooth fundamental wave forms delineated in the Paper to which reference has already been made‡ are quite inadequate to produce the amount of disturbance which is in fact experienced in radiotelegraphic receivers. This was sufficiently obvious from the beginning, and the reference in the Paper to short period ripples should not have been overlooked as an explanation of the discrepancy. It may, however, be permissible to expand the evidence from more recent observations.

In the first instance it may be remarked that no results were, in the Paper, quoted from measurements on an alternative, and in many respects more attractive, circuit in which the ordinates of the oscillogram are proportional, not to the instantaneous value of the field change constituting the atmospheric, but to the first differential of that quantity, i.e., to the rate of change of field. The reason for this

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* Watson Watt, "Directional Observations of Atmospherics—1916-1920," Phil. Mag., 45, 1010 (1923). Watson Watt, "Directional Observations of Atmospheric Disturbances—1920-1921," Proc. Roy. Soc., A., 102, 460 (1922).

† Moullin, 'Atmospherics and their Effects on Wireless Receivers," Jour. I.E.E., 62, 353 (1924). Eckersley, "The Energy of Atmospherics," Electrician, 93, 150 (1924).

‡ Watt and Appleton, loc. cit.

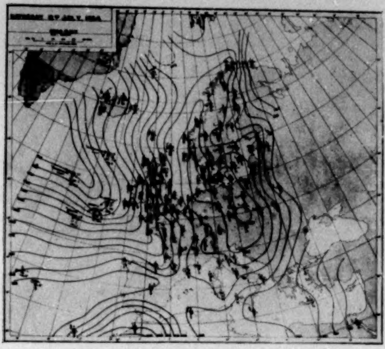

FIG. 4A.

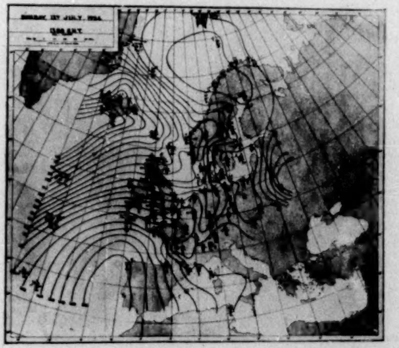

FIG. 4B.

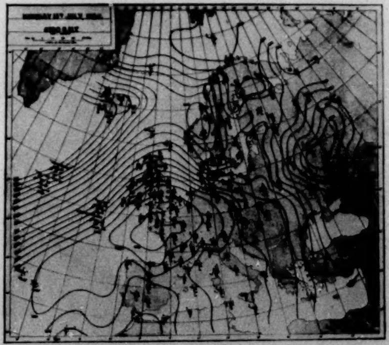

FIG. 4C.

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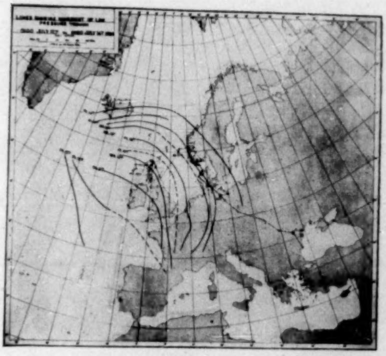

FIG. 5.

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was mainly that the fine structure of many atmospherics, even in the comparative quiet of a British winter, was so well-developed that the oscillogram of rate of change of field was too complex to be delineated or resolved with the apparatus and experience then available.

Experience in the tropics and in sub-tropical regions immediately revealed the fact that this fine structure increased rapidly in relative amplitude with decreasing latitude, and was particularly prominent during the hours of darkness. A fuller investigation of this phenomenon must await the reduction of the wave form data collected, but some preliminary notes on the details of this fine structure, as observed at Khartoum in April, 1924, may be of immediate interest.

In low latitudes, and at night, the ripples rise in importance from 10 per cent. relative amplitude on less than 10 per cent. of all forms observed, as was the case in the first series of wave-form determination, to a relative amplitude varying from 10 per cent. up to 100 per cent. on not less than 70 per cent. of all individual forms examined on a sufficiently open time base.

Figs. 6 to 8 (not reproduced) are untouched photographs of the original drawings of a few typical dark-hour atmospherics from Khartoum, showing, successively, details of wave structure observed on time bases of 250 and 500μs, typical oscillograms of the rate of change of field from lightning within visible range, an oscillogram of the change of field from lightning just beyond visible range, and a random page of observations on a base of 1,000μs.

VII. Conclusion.

In conclusion, the present position of the geophysical study of atmospherics may be summed up as follows. The observational methods available for the study of wave form are, as developed by Appleton, Herd, and Watt, such that detailed examination of components whose duration is of the order of 100μs may be carried out with ease, while observation down to 25μs can be carried out. The necessary instrumental improvements to increase resolving power in this direction to a limit certainly below 10μs are attainable without further experiment, and are being undertaken. The existing apparatus and methods are sufficient to enable a specification to be drawn up, from sampling observations, of the total number per unit time of atmospherics passing specified limits of field strength and rate of change of field, and of the relative distribution of types, in any region. The origin of the prominent atmospherics received in Great Britain has been traced to the European meteorological situation ; improved means have been devised for the prosecution of such studies and are being applied as part of the fundamental study of the nature and origin of atmospherics which is being carried out by the Radio Research Board. Like all geophysical studies this investigation cannot be completed without cooperative effort ; a start has been made within the British Empire, but there is no doubt that the time is close at hand when European collaboration will be essential to progress.

Thanks are due to the Radio Research Board established under the Department of Scientific and Industrial Research, and in particular to its Committee on Atmospherics, for the provision of facilities for the work outlined in this Paper, and for permission to publish the results. Thanks are also due to the bodies and individuals mentioned in the Paper, for facilities, active help, and collaboration.

THE ELECTRIC FIELD OF A THUNDERCLOUD AND SOME OF ITS EFFECTS.

By C. T. R. WILSON, *F.R.S.*

ABSTRACT.

A thundercloud is an electric generator in which the separation of positive and negative charges occurs at a rate corresponding to a current which may amount to some amperes; the potential difference between its poles may reach values of the order of a million kilovolts. Attention is drawn to three effects of the electric field of thunderclouds or shower clouds (all of which may occur even when there is no thunder). (1) The electric field of the cloud may cause ionization at great heights, the result being continuous or discontinuous discharge between the cloud and the upper atmosphere. (2) Discharge from pointed earthed conductors is likely to constitute an important part of the current between the ground and the base of a thundercloud, and the resulting ionization near the ground may be large. (3) By its accelerating action on particles the electric field of a thundercloud may produce extremely penetrating corpuscular radiation.

INTRODUCTION.

WE may regard a thundercloud as a great electric generator. I shall not attempt to discuss its probable mechanism, whether, for example, it resembles more closely that of a frictional or of an influence machine. The principal theories of its action are those of Simpson and of Elster and Geitel.

Observations on lightning discharges and on the changes in the electric field which are associated with them indicate that the rate of separation of positive and negative charges within a thundercloud frequently corresponds to a current of some amperes, and that potential differences of the order of one million kilovolts are developed.

The charges separated in the thundercloud may re-combine directly by a short-circuiting discharge within the cloud or by continuous or discontinuous discharges through external circuits, one such circuit including the earth and the upper atmosphere.

The changes produced in the electric field by lightning discharges at different distances are most easily interpreted if we suppose that the upper charge of a thundercloud may be either positive or negative, but that it is more frequently positive than negative.

The most conspicuous effect of the electric field of a thundercloud is the production of lightning discharges. I do not propose to discuss the phenomena of ordinary lightning discharges; I shall confine myself to the consideration of three less obvious effects which may result from the electric field of a thundercloud. These effects are: (1) ionization in the upper atmosphere; (2) ionization by point discharge from earth-connected conductors; and (3) the production of penetrating radiation.

I. IONIZATION IN THE UPPER ATMOSPHERE BY THE ELECTRIC FIELD OF A THUNDERCLOUD.

Each of the charges (upper and lower) of the thundercloud forms with its image in the earth a system which has an electric moment $2\Sigma qh=2QH$, where q is the charge at any height h, Q is the whole charge, and H its mean height above the ground. The electric moment of the thundercloud is the difference between the electric moments of its upper and lower charges. (Where the electric moment of a charge, or of the whole cloud, is spoken of it is to be understood that the effect of the image is included.) At distant points on the earth's surface (not, however, so distant that the curvature of the surface has to be taken into account) the vertical electric force due to a thundercloud of moment M is M/r^3, where r is the horizontal distance of the point from the centre of the thundercloud. The sudden changes in the electric moment of the thundercloud which result when one or both charges are wholly or partially destroyed by lightning discharges may be found by observing the sudden changes in the vertical electric force at the ground at known distances. The mean value of such changes in the electric moment of thunderclouds, or, in other words, the mean value of the electric moment of a lightning discharge, is found to be of the order of 3×10^{16} e.s.u. cm. The average electric moment of a thundercloud on the point of discharge is likely to be greater.

The electric force due to a cloud of electric moment M, at a point vertically above it in the upper atmosphere, may be taken as approximately $2M/r^3$, where r is the height of the point above the ground. While the electric force due to the thundercloud falls off rapidly as r increases, the electric force required to cause sparking (which for a given composition of the air is proportional to its density) falls off still more rapidly. Thus, if the electric moment of a cloud is not too small, there will be a height above which the electric force due to the cloud exceeds the sparking limit.

At a height of 60 km. the density of the air is about $1{\cdot}6\times10^{-4}$ of that near the ground, while the composition of the air is not very different, so that the critical value of the field may be taken as about $30{,}000\times1{\cdot}6\times10^{-4}=4{\cdot}8$ volts per centimetre. To produce such a field at this height a thundercloud would require to have an electric moment of $1{\cdot}7\times10^{18}$ e.s.u. cm.; to produce a field of the critical value at greater heights a smaller electric moment would be sufficient, but the data as to composition and density become uncertain. If we assume the critical field to remain proportional to the pressure, a thundercloud with an electric moment of about 1/10 of the above value (i.e., only a few times the average electric moment of a lightning flash) would produce a field exceeding the critical value at a height of 80 km. Thus, if there were no already existing conducting layer there is little doubt that a thundercloud would itself cause ionisation in the upper atmosphere.

Let us assume that there is already a conducting layer with its lower boundary at, for example, 80 km. Ions are dragged out of this by the field of the cloud, positive if the upper pole of the cloud is negative, negative (probably as free electrons) if it is positive. If the field exceeds a critical value, considerably less for the negative than for the positive ions, the ions acquire sufficient energy to cause ionization by collision. If the field is strong enough to cause ionisation by collision for any considerable distance below the original level of the conducting layer, it is possible that sudden discharge of the upper pole of the thundercloud to the upper atmosphere may occur.

The presence of the conducting layer increases the vertical electric force at points below it. Again, the value taken for the critical field (the sparking value) corresponds according to Townsend's theory to ionization by collision by positive ions; a smaller field is sufficient to give to negative ions the energy necessary for ionization. Thus the estimate given above for the electric moment required to cause discharge at 60 km. is probably an excessive one, especially when the cloud is of positive polarity.

The vertical electric force at a point at a height of 60 km. may be due, not solely to a thundercloud vertically below it; all thunderclouds within a considerable area will contribute to it. A cloud 10 km. away from the centre of this area would produce at the given point a vertical force exceeding 9/10 of that which it would produce if it were at the centre. It is possible that the electrical field of a large rain cloud, which would not be regarded as a thundercloud, may be strong enough to cause discharge in the atmosphere above it. An average electric moment of about 5×10^{15} per sq. km. would be more than sufficient to produce discharge at 60 km. if the cloud covered a circular area of 10 km. radius. This value of the electric moment may easily be attained while the electric force within and below the cloud remains far short of the sparking value. The discharges above the cloud would doubtless give rise to atmospherics. If, as has been maintained, atmospherics frequently originate in regions of rain unaccompanied by thunder, they may in such cases be due to discharges of this nature.

II. Ionization by Point Discharge from Earth-connected Conductors.

The electric field at the ground below a thundercloud very often exceeds 10,000 volts per metre, and below the centre of the cloud it probably considerably exceeds this value; observations made in a properly exposed situation below the centre of a thundercloud are naturally difficult to obtain. Even below rainclouds without thunder potential gradients of several thousand volts per metre are common. Any pointed earth-connected conductor, such as a lightning conductor, projecting to a height in a field of a few thousand volts per metre must give rise to a glow or brush discharge, and a conductor need not end in a sharp point or project to a great height in order that it should begin to act as a discharger. A very simple calculation is sufficient to show that an earth-connected sphere of 1 cm. radius need only be raised to a height of 3 metres in a field of 10,000 volts per metre in order that the electric force at its surface may reach the critical value of 30,000 volts per cm. That considerable currents should be obtained in experiments like that made long ago by Benjamin Franklin with his kite, and that " St. Elmo's fire " should occur in exposed situations, is not therefore surprising.

A considerable part, possibly the greater part, of the current between a thundercloud and the ground must be carried by trees and other natural lightning conductors. It is even more difficult to obtain measurement of this portion of the current (that down a tree, for example) than of the charges carried by rain or by lightning. Useful information bearing on the matter—and particularly on the very fundamental question whether the current between the ground and a thundercloud is on the whole mainly upward or downward—is likely to be obtained by observations with lightning conductors erected for the purpose and connected to instruments for recording the current through them. Observations of the sign and magnitude of the intense potential gradients below thunderclouds will also help towards obtaining some estimate of the currents. I have lately been testing methods for making both types of observations.

Some years ago I made experiments to determine what is the magnitude of the potential gradient which has to be applied over the surface of a field of grass to make the tips of the grass blades come into action as point dischargers. With a positive potential gradient, i.e., with the grass negatively charged, the discharge from a square metre of the grass-covered ground begins to be measurable when the applied potential gradient reaches about 15,000 volts per metre, and the current increases with great rapidity as the potential gradient is further increased; it soon reaches values of the order of 1 micro-ampere per sq. metre, i.e., of 1 ampere per sq. kilometre. A somewhat larger potential gradient, about 20,000 volts per metre, is required to start the point discharge when the ground is positively charged, i.e., when the potential gradient is negative, and a larger gradient is required to produce a given current. This difference is in accordance with what is known of positive and negative point discharges.

It is of interest to consider the vertical current due to such discharge from grass-covered ground below a thundercloud under ideally simple conditions. Let us suppose that no rain is falling and that the whole current between the ground and the cloud is carried by ions supplied by point discharge from the grass. To simplify the problem, suppose that we have immediately below the thundercloud a vertical field which is uniform over an area of dimensions great compared with the height of the base of the thundercloud. Let us suppose that this field approaches the sparking limit in intensity, and that it is directed upwards, the base of the cloud being positively charged.

If an electric field of this intensity extended as far as the grass-covered ground, this condition could only last momentarily, since a continuous discharge of negative electricity would at once occur from the grass until the potential gradient at the ground only slightly exceeded that required to maintain point discharge from the grass. If a steady condition is reached there must be a streaming of negative ions upwards from the ground to the positively charged base of the cloud; the space between the ground and the cloud will then contain a negative volume charge and the field will increase with increasing height above the ground. If, in spite of the upward stream of negative ions the electric force immediately below the cloud is maintained at a high value, this can only mean that the positive charge at the base of the cloud is being replenished (by the source of E.M.F. within the cloud) as fast as it is being neutralised by the upward streaming negative ions. The conditions are not essentially different when the potential gradient is negative, so that the upward streaming ions are positively charged.

If k is the mobility of the upward streaming ions (their velocity under unit electric force), ρ the charge carried by the ions in 1 cubic cm. of the air, F the vertical electric force, h height above the ground, and γ the vertical current per square centimetre, then, since

$$\gamma = k\rho F$$

and

$$\frac{dF}{dh} = 4\pi\rho = \frac{4\pi\gamma}{kF},$$

$$F_h^2 - F_1^2 = \frac{8\pi\gamma h}{k}$$

F_h and F_1 being the vertical electric force at a height h and at the surface of the ground respectively.

The vertical electric force F_1 immediately over grass-covered ground cannot much exceed 30,000 volts per metre (=1 unit of electric force in e.s. measure).

Let us assume that just below the cloud the electric force approaches the sparking value, 30,000 volts per centimetre, or 100 in e.s. measure. Then at all except quite small heights, F_1 will be negligible in comparison with F_h, and we have approximately

$$F_h^2 = \frac{8\pi\gamma h}{k} \text{ or } \gamma = \frac{F_h^2 k}{8\pi h}.$$

Assuming that the base of the cloud is at 2 km., and that the vertical force there almost reaches the sparking limit, and that the mobility of the ions is that of the ordinary small ion, we find for the magnitude of the vertical current about 6 e.s.u. per square centimetre per second or 2 amperes per square kilometre. The number of ions per cubic centimetre (of mobility of the order of 1 cm. per second for a field of 1 volt per centimetre) would vary from something of the order of 3×10^6 near the ground, where the field is of the order of 300 volts per centimetre, to about 1/100 of this just below the cloud.

The current and ionization found above are those which would exist if the electric field below the thundercloud were maintained almost up to the sparking limit. The field immediately below the cloud could not, as a result of separation of charges within the cloud, reach the limit required for a discharge to earth, unless the rate of separation of charges exceeded the value found above for the current. Thunderstorms, in which discharges pass at frequent intervals between the base of the cloud and the ground, are, of course, quite common. It would, however, be unsafe to assume that the conditions in such storms are necessarily like those we have assumed. It is, for example, possible that the field below the cloud may only approach or reach the sparking value as a result of discharges of the upper pole of the nature of those discussed in section (I); the large currents and ionizations would in such cases only last for a very short time.

III. PRODUCTION OF PENETRATING RADIATION.

As I have elsewhere pointed out,* intense electric fields which, like those of thunderclouds, extend over considerable distances, may have important effects if β-rays are present. A β-particle is, under normal conditions, continually losing energy in ionizing and otherwise affecting the atoms which it traverses. The rate of loss of energy per centimetre of path is less the faster the particle is moving, varying approximately as the inverse square of the velocity. In air at normal pressure the mean rate of loss of energy per centimetre for a particle of which the kinetic energy is the equivalent of 20,000 volts (range about 1 cm., velocity about 9×10^9 cm. per second), is about 10,000 volts per centimetre. A field of 10,000 volts per centimetre directed along the path of such a particle would, on the average, just compensate for this loss, and would more than compensate for the loss and give a balance of acceleration if its velocity were any greater. In a field of 20,000 volts per centimetre (only two-thirds of the sparking field) the energy gained by a 20,000 volt β-particle would exceed the loss by about 10,000 volts per centimetre. The faster the β-particle moves the smaller is the rate at which it loses energy in collisions; the rate of loss will approach a limit of less than 1,000 volts per centimetre as the velocity becomes comparable with that of light. The chances of accidental deflections out of the accelerating field also become less and less as the energy of the particle increases.

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* Camb. Phil. Soc., Nov. 24 (1924).

There can be no doubt that β-particles are continually being emitted in the strongest parts of the field of a thundercloud (where the field approaches the sparking limit), as elsewhere in the atmosphere. A considerable proportion of these, and of others secondarily produced, must under the influence of the field acquire large additional energy. The greater the energy acquired by the particle the greater is its chance of surviving to gain more energy. The net rate of gain of energy by a particle which has travelled more than a few centimetres in the strongest part of the field may exceed 10^4 volts per centimetre, or 10^6 volts per metre. Thus, β-particles which have traversed a few metres in the direction of the field have already acquired energies exceeding those of the fastest known β-particles from radioactive substances. And the energy gained from the field will continue to exceed that lost in collisions even when the particles have reached regions where the field has fallen to 1,000 volts per centimetre.

A particle may thus acquire energy corresponding to the greater part of the whole potential difference between the poles of the thundercloud, which may be of the order of 10^9 volts. Such a particle may be expected to have important effects (when, for example, it strikes the nucleus of an atom), as its mass will be comparable with that of the hydrogen nucleus.

It has been assumed above that the air is at atmospheric pressure. The results remain essentially the same at low pressures; the maximum electric force which can exist in the air without discharge is proportional to the density, while the length of path required for a given energy loss by a particle of given velocity is inversely proportional to the density. For corresponding fields (which are proportional to density) the net gain of energy in corresponding distances (which are proportional to the density) remains the same; the linear dimensions of the β-ray system are inversely proportional to the density. At great heights where the electric force is much reduced the deflecting action of the magnetic force becomes important, and the β-rays will tend to run mainly along the direction of the magnetic lines of force.

The corpuscular radiation originating in the field of a thundercloud and the γ radiation which it may excite will constitute a penetrating radiation very similar in character to that which is known to occur normally in the atmosphere.

THE EVIDENCE OF TERRESTRIAL MAGNETISM FOR THE EXISTENCE OF HIGHLY IONIZED REGIONS IN THE UPPER ATMOSPHERE.

By Prof. SYDNEY CHAPMAN, *M.A., D.Sc., F.R.S.*

ABSTRACT.

Balfour Stewart's "atmospheric dynamo" theory of the daily magnetic variations is described; the earth's permanent magnetic field is the magnetic field of the dynamo; the atmosphere (undergoing small, but widespread, motions owing to thermal or tidal causes) is the moving armature; and particular ionized regions in the upper strata correspond to the "windings" in which the induced currents flow. The variations of ionization are the chief cause of the changes of intensity in the diurnal magnetic variations, as between day and night, winter and summer, and sunspot minimum and sunspot maximum. Hence these changes in the magnetic variations indicate the changes in the ionization of the atmospheric regions concerned. It is suggested that there are two modes of ionization, both due to the sun, and affecting different regions; one region, ionized by ultra-violet radiation, is a practically world-wide layer, possibly at about 50 kilometres above the surface; this is most intensely ionized directly under the sun in the sun-lit hemisphere, and the ionization diminishes during the night. The other region consists of the polar caps within the auroral zones, and especially these zones themselves; this region is ionized by the impact of corpuscles emitted from disturbed regions on the sun's surface.

I.

THE earth's magnetic field, and its variations, are known from observations in many parts of the earth; these observations are practically confined to the earth's surface. Very little of this field can be ascribed to currents flowing across the earth's surface or in the immediately adjacent atmosphere; hence the field has a potential in this region. By spherical harmonic analysis we can determine what proportion of the field originates within the earth, and what originates above the surface. The main "permanent" or secularly varying field is thus shown to be almost wholly of internal origin. The same method can be applied to the superposed fields which are responsible for the "rapid" magnetic variations, using the word rapid to denote the more or less regular *daily* variations, as well as the irregular changes known as magnetic disturbance (or, in extreme cases, magnetic storms). The analysis—first made by Schuster—shows that these rapidly varying fields are partly internal and partly external in origin, but that the external portion is from twice to thrice as great as the internal one. The internal part can be explained satisfactorily as a secondary effect of the external part, being produced by the currents induced within the earth by the varying outer field. The conductivity of the earth required to account for the numerical relationships between the two fields is of reasonable magnitude, and estimates derived from different components of the varying field are in good agreement. The theory of these variations is thus reduced to that of the external part of the fields.

II.

Many causes have been assigned for these varying external magnetic fields, and no theory, as yet, is without its difficulties. So much is required, for any theory to be regarded as at all satisfactory or promising, that but few of those proposed have survived even a brief examination. The magnetic fields to be explained are specified by three variables (the north, west, and vertical components of magnetic force) at each place, and each of these variables is a function of latitude, longitude and time; any theory of their origin must co-ordinate these numerous facts, must indicate the situation, form and changes of the current-systems (lying somewhere above the earth's surface) which produce the fields, and must explain the causation of these currents. Many of the postulates contained in such a theory must, unfortunately, be incapable of direct confirmation by observation and measurement, whether the currents are supposed to flow outside the atmosphere or within it; for their intensity, as a simple calculation readily shows, is necessarily so considerable that even if they flow within the atmosphere it can only be in highly rarefied regions near or beyond the present limits of accessibility. None the less, the facts to be explained are so many and varied that any theory which by fairly simple hypotheses can satisfactorily account for all, or nearly all, is unlikely to be substantially wrong.

III.

Balfour Stewart gave the essence of what is now the generally accepted theory of the daily magnetic variations, and the subsequent great advances in our knowledge of physics in general, and atmospheric physics in particular, have steadily added support to what at the time was a bold piece of speculation. According to this theory, the currents which produce the daily varying magnetic field flow in the atmosphere, and therefore, considering the relatively small depth of the air, the direction is more or less parallel to the earth's surface; and they are induced by large-scale motions of the atmosphere in the presence of the earth's permanent magnetic field. The electromotive forces thus induced can take effect, however, only in some highly conducting region of the atmosphere.

IV.

On this view, three main factors determine the character and intensity of the electric currents and their associated magnetic fields. The first is the earth's permanent magnetic field, which may be considered definitely known in intensity and distribution throughout the atmosphere. It is, on the whole, a very simple field, almost the simplest possible over a spherical surface; but it does, of course, vary greatly in intensity and inclination from one pole to the other, and these variations must powerfully affect the currents arising from any given atmospheric circulation. They are in this way of great assistance in providing checks on the general soundness of the theory; the observed facts agree well with such inferences as can be drawn from these considerations.

V.

The second factor in this theory of the "atmospheric dynamo" is the large-scale motion of the atmosphere, which may be likened to the motion of the armature in the field of the permanent magnet provided by the solid earth. The third factor is the conductivity of the atmosphere. The ordinary winds in the atmosphere must themselves produce quite appreciable electromotive forces, but the electrical conductivity near the ground is too small for these forces to produce appreciable electric currents. The layer where the necessary conductivity exists must be at a considerable height in the atmosphere. The motion of the atmosphere at great heights is not known so definitely as the earth's magnetic field at such levels, but we have, at any rate, some knowledge of the forces acting on the atmosphere, and by means of wind and barometer observations near the ground we can infer something as to the motions higher up. Moreover, we are concerned only with the largest features of the atmospheric motion, because relatively local circulations have a smaller magnetic effect than the world-wide movements.

VI.

The case in which we can be most sure of the atmospheric motions is that of the lunar daily magnetic variation. If the "atmospheric dynamo" theory is correct, and we rule out (as is easily justified) any direct magnetic effect of the moon of the required kind, the only way in which it seems possible for the moon to act is by producing a tidal circulation in the atmosphere. The character of such a circulation can be inferred by dynamical theory: the pressure is increased to a maximum at the two ends of the diameter of the earth which at any given instant passes through the moon, and at other points on the earth the flow of air is towards or away from the nearer of these two points; the variations of pressure and motion are similar over the two halves of the earth visible and invisible from the moon—i.e., they are semidiurnal in character. Moreover, as the moon varies in distance from the earth, the tide-producing force varies as the inverse cube of the distance, the whole variation being nearly 40 per cent.; the tidal variations of wind and pressure should show similar changes. By the analysis of barometric observations the existence of a circulation showing these characteristics has been established.

VII.

The character of the electric current system and magnetic field corresponding to such a tidal circulation can be determined if we assume that there is a thin spherical layer in the atmosphere, surrounding the earth, of uniform conductivity, in which the electromotive forces generated by the atmospheric dynamo can take effect. The assumption that the conductivity is uniform is incorrect, as will appear later, but the predicted type of lunar daily magnetic variation agrees surprisingly well with the observed type, derived from the mean of one or more month's data. In particular, the observed variation as so obtained is semidiurnal, each force component at each station having two equal daily maxima and two equal daily minima at intervals of a quarter of a day. Moreover, the magnetic variation increases from lunar apogee to lunar perigee, as the moon's distance varies from its maximum to its minimum, in a similar way to the tidal circulation itself. This is strong evidence of the general truth of the theory, and justifies the further step of calculating the conductivity of the atmospheric layer from the known data, viz., (i) the earth's permanent magnetic field, (ii) the tidal motion of the atmosphere, and (iii) the observed intensity of the lunar daily magnetic variation. The calculated value indicates that the layer must be about equal in conductivity to a layer of copper, at ordinary temperature, 1 metre thick.

VIII.

This result is confirmed, as regards its order of magnitude, by the solar diurnal magnetic variation. The sun produces in the earth's atmosphere a circulation similar in type to the tidal circulation due to the moon ; in the case of the sun the circulation is partly due to thermal and partly to tidal causes. This circulation should, on the present theory, produce electric and magnetic effects similar to those caused indirectly by the moon ; this is found to be the case. The solar diurnal magnetic variation is about ten times as intense as the lunar diurnal magnetic variation, agreeing fairly well with the fact that the solar diurnal atmospheric circulation is about fifteen times as rapid as the lunar tidal movements. If, therefore, the corresponding electric current systems flow in the same atmospheric layer, the estimates of the conductivity of this layer derived in the two cases will be nearly equal.

IX.

The solar diurnal magnetic variation, however, is not semidiurnal in type (that is, the same in any two hemispheres separated by any plane through the earth's axis), although the atmospheric circulation which produces it has this character, like the lunar tidal circulation. The daily magnetic variation field is more intense during the day than during the night hours, that is, over the sunlit than over the dark hemisphere. This is a direct indication that the conductivity and ionization of the atmospheric layer in which the induced currents flow is greater over the sunlit than over the dark hemisphere.

X.

The lunar diurnal magnetic variation shows the same intensification during the hours of sunlight in a still more striking way. It has been already stated that in the mean of one or more months this magnetic variation, like the tidal circulation which produces it, is purely semidiurnal. But at any particular epoch in the month this is no more true than it is of the solar diurnal magnetic variation : both are intensified during the hours of sunlight. These hours occur at different lunar times in the course of the lunar month, owing to the motion of the moon round the earth ; each lunar hour occurs at all solar hours equally in the course of the month, and receives an equal share of sunlight and darkness. Hence in the mean of a month the lunar daily magnetic variation has a purely semidiurnal character, though at no individual epoch is it of this type. In the lunar daily magnetic variation the moon governs the atmospheric circulation and the sun the ionization of the layer in which the induced currents flow, and owing to the relative motion of the two bodies their two functions are more clearly separated than in the case of the solar diurnal variation, in which the sun governs both the circulation and the ionization of the atmosphere.

XI.

One important remark remains to be made about the first two of the three factors which determine the action of the atmospheric dynamo, viz., the permanent magnetic field and the atmospheric circulations. Neither of them is liable to any considerable change either irregularly from day to day or systematically from sunspot minimum to sunspot maximum. Consequently any considerable day-to-day change in the solar or lunar daily magnetic variations, or any considerable systematic change in the course of the solar cycle, must be due to the variation of the third factor, viz., the ionization and conductivity of the layer or layers in which the induced currents flow. This conclusion is of great importance, because it indicates a powerful means—almost, if not quite, the only one at our disposal—of estimating the variations of ionization at high levels in the atmosphere. It leads also to the inference that in spite of their great similarity, the solar and lunar diurnal magnetic variations are produced (partly, at any rate) in different ionized regions of the atmosphere.

XII.

In the middle belt of the earth, extending on both sides of the equator* and including the major part of the earth's surface, the solar diurnal magnetic variation (which for brevity will hereafter be referred to as S) is very much the same on magnetically quiet (or undisturbed) days as on average days at the same season. There is a difference between the two, but it is only a slight one, and it is very nearly true to say that S is the same from day to day, independent of the magnetic activity (i.e., irregular magnetic disturbance), except when the latter becomes unusually great.

XIII.

The behaviour of the lunar diurnal magnetic variation (which will be denoted by L) is in this respect quite different from that of S; the amplitude of L undergoes large changes from day to day, in general increasing or decreasing in unison with the magnetic activity. At some stations (in the middle belt of the earth) and in some force components, the amplitude of L on the five most disturbed days per month is fourfold as great on the five quietest days per month. These changes are quite without a parallel in S in the middle belt of the earth, and if, as is here concluded, they are due to variations of ionization in the layer or region where L is generated, this region must be different from that in which S is produced.

XIV.

There is a further difference between S and L which indicates the same conclusion. While S does not vary from day to day at the same season, it varies considerably from year to year, its amplitude at sunspot maximum being 40 or 50 per cent. greater than at sunspot minimum, with but little change of type. The ionized layer in which S is produced must therefore vary in conductivity by this amount, and since the ionization is proportional to the square root of the intensity of the ionizing agent, the latter must be nearly twice as great at sunspot maximum as at sunspot minimum. On the other hand, it seems now to be definitely established that the average value of L varies only slightly from sunspot minimum to sunspot maximum, probably by less than 10 per cent.; the fact is the more remarkable because disturbed days, on which L (at any epoch) is increased, are undoubtedly more numerous at sunspot maximum than at minimum, and this in itself seems sufficient to account for the small observed increase of L at maximum epoch.

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* So far north, for example, as Greenwich or Kew.

XV.

Beyond the middle belt of the earth, in the polar regions, irregular magnetic disturbance or activity is much more intense than in lower latitudes, though its proportionate changes of intensity are, on the whole, common to the earth as a whole. Magnetic disturbance has its origin in the polar regions, and in the middle belt of the earth it is seen only in an attenuated form. Intimately associated with the irregular disturbance is the special form of the solar diurnal magnetic variation observed in the polar regions; this, like L, varies greatly from day to day in rough proportion to the degree of disturbance, and it is, in fact, a much attenuated extension of this portion of S into the middle belt of the earth that produces the slight differences, already mentioned, shown by S in this region as between quiet, average and disturbed days at the same season and solar epoch.

XVI.

A detailed study of the daily magnetic variations in the polar regions convinces me that over each polar cap there is in the upper atmosphere a roughly circular electric current circuit, which seems usually to correspond approximately in position with the zone of maximum auroral frequency. The latter zone is nearly circular, with its centre near the pole of the axis of magnetization of the earth, and with an angular radius of about 23°. Since auroral and magnetic disturbance are very closely associated, the two zones are probably identical, in which case the current must flow in the belt ionized by the electric particles from the sun which give rise to the aurora. This implies that the current is upwards of 90 kilometres above the surface of the earth. The current appears to flow from east to west, but to be of unequal intensity round the zone, some current finding its return flow over the polar cap from one point of the belt to the other, nearly parallel to the meridian plane through the sun. Thus, while the zone is specially conducting, the whole polar cap within the zone would also appear to be ionized at a similar level but to a lower degree.

XVII.

The conception which I have formed of the larger features of the electromagnetic phenomena in the polar regions and elsewhere in the upper atmosphere is different in some important respects from one which I described a few years ago in a Paper entitled "An Outline of a Theory of Magnetic Storms." It can only be sketched very briefly here, prior to the publication of several investigations, partly completed and partly in progress, giving in detail the data and the arguments in support of these conclusions. At the basis of the theory is the hypothesis that in the polar regions there is an atmospheric zone or ring of very variable and intense conductivity, associated with auroræ. A poleward motion of the atmosphere at auroral heights in this latitude would supply electromotive forces of the right type to account for the westerly current along the zone which has already been mentioned. Very little direct evidence as to the motion of the atmosphere at auroral heights is available, but a provisional estimate of the velocity of the poleward motion proves to be of not excessive magnitude. The lunar tidal circulation must also extend to auroral heights, and it seems probable that its current production reaches its maximum in the auroral belt, owing to the high average value of the conductivity there.

XVIII.

The currents produced in the polar regions by these two sets of atmospheric motions must be accompanied by relatively intense magnetic fields in the neighbourhood of the zones, but in the middle belt of the earth these fields are almost inappreciable. The disturbance field and the lunar diurnal variation field observed in the middle belt of the earth appear to be the fields of induced currents, flowing partly in the earth and partly in the atmospheric ionized layer, extending over the greater part of the earth, in which S is produced. That varying currents in the polar regions could by induction in the atmosphere and the earth produce currents which would have considerable magnetic effects in the middle belt of the earth is not by any means obvious, but it is a conclusion indicated by calculations, necessarily of a rather complicated nature, in which Mr. T. T. Whitehead has co-operated with me.

XIX.

As regards L, this theory is a modification of the simple theory previously explained. The actions there described must occur, but it would seem that the L-field so produced is a minor part of the whole, and that the major part of the current system associated with L which flows in the ionized layer over the middle belt of the earth is a secondary system induced by primary currents flowing in the independent highly-ionized polar regions. Many questions raised by this revision of the theory of the lunar diurnal magnetic variation are at present under examination.

XX.

The conclusions relative to the ionization of the upper atmosphere, which can be drawn from the evidence (here briefly and inadequately described) of terrestrial magnetism, may be summed up as follows : There are two independent regions of high conductivity, ionized by independent solar agencies. One of these regions is a layer extending nearly or quite over the whole earth ; its conductivity is greatest where the sun's zenith distance is least, and therefore it varies throughout the day and night at any one station ; when the station is in the sunlit hemisphere the conductivity is much greater than during the night hours. The average conductivity of this layer is comparable with that of a layer of copper, under ordinary conditions, one metre thick. The conductivity increases considerably (by 40 to 50 per cent.) from sunspot minimum to sunspot maximum. The distribution of conductivity in this layer suggests that the ionizing agent is ultra-violet radiation ; it seems likely that the ionization is associated with the production of the layer of ozone which is known from the work of Fowler and Rayleigh to exist in the upper atmosphere. Consideration of the absorption of the ozone bands of ultra-violet radiation indicates a height of about 40 or 50 kilometres for this layer ; the *magnetic* evidence does not suffice, so far as the theory is developed at present, to indicate the height of the layer.

XXI.

The other ionized regions are the auroral zones round each pole, and the polar caps within these zones. The polar situation of the zones is satisfactorily explained by the theory of Birkeland and Störmer, viz., that it is due to the deflection in the earth's magnetic field of streams of charged particles coming from the sun, which must necessarily enter the atmosphere in a fairly limited region round each pole. Measurements of the height of auroræ indicate that these particles penetrate the atmosphere down to about 90 kilometres above the ground ; the auroræ are observed up to a height of several hundred kilometres, so that the zone of ionization must be very deep, though the greatest conductivity probably occurs near the lower limit. It should be possible, by means of suitable magnetic data, to determine the average total conductivity of this zone, but at present this has not been done.

XXII.

There is good evidence that the streams of corpuscles which ionize the auroral zones and polar caps are associated with disturbed regions on the sun, and that the irregular occurrence of magnetic disturbance is due not only to the variability of these solar regions, but also to the rotation of the sun, which constantly changes the direction of emission of the streams. The ultra-violet radiation which ionizes the world-wide atmospheric layer probably proceeds from the solar surface as a whole, and its variation throughout the solar cycle indicates that the sun's surface in general, and not only the specially active parts of it, undergoes important changes in the course of the solar cycle.

GENERAL DISCUSSION.

Dr. G. C. **Simpson** maintained, with regard to Mr. Wilson's contribution, that no experimental or observational evidence has yet been produced to show that thunderstorms ever have negative electricity under positive electricity, and until this is done he is unable to accept Mr. Wilson's explanation of the origin of the normal negative charge on the earth's surface. Prof. Chapman's theory is open to the objection that the air-tides produce only very small differences of pressure even at the surface of the earth, while at the height of the conducting layer which he postulates (50 km.) such differences would be minute. They could hardly, therefore, influence the motion of the atmosphere to the extent demanded by his view of the matter. In connexion with Prof. Appleton's Paper, it was to be noted that the strength of signals received from a sea station had been observed at Slough and at Orfordness, the latter station lying about half-way between the other two. During the daytime there was no substantial error in the direction found by either of the land stations. At night there was no error at Orfordness, where the signal was received purely over the sea; but at Slough a large variation in intensity and apparent direction was introduced by the land passage. It seems incredible that for three stations so close together such a marked difference could be caused by a conducting layer 60 km. up, and the latter also failed to account for the effect of the change from daylight to dark. Mr. Watt had shown that atmospherics may originate where there is no thunderstorm, but an atmospheric demands an electric discharge of some sort. What can be the nature of such a discharge in a cloud which is not a thunder-cloud ?

Prof. C. L. **Fortescue**: The opening Paper by Dr. Eccles and the subsequent Paper from Prof. Appleton have epitomised the existing knowledge with regard to the Heaviside layer and the part which it plays in the transmission of wireless signals. Neither of these two speakers, however, has dealt with the question of the sudden transition from high conductivity to low conductivity which appears to be required if this layer is to play the part assigned to it. Dr. Wilson's contribution suggested a possible explanation of a sudden change in the conditions of the upper atmosphere. He pointed out that above the charged cloud the electric field distribution was one which made it possible for ionization to take place in the upper air, and he suggested that this ionization might be either of a stable or unstable type. In the former case it would appear that the change from the ionized state to the unionized state would be sharply defined, and the conditions would be those necessary for the Heaviside layer as assumed. This possible explanation, of course, assumes that there are charged zones in the lower atmosphere. In view of the observed high voltage gradients at all times it seems probable that either these charged layers are always present, quite irrespective of the phenomena of thunder and lightning, or that corresponding electric fields are set up in some other way.

Mr. Watson Watt's Paper is of great interest to wireless engineers who have endeavoured to find an explanation of the devastating effects of atmospherics in practice. In his earlier Papers he describes wave forms in which the time interval was several milli-seconds. In this present Paper he has shown wave forms of time interval not exceeding 25 micro-seconds, and many of these wave forms have a complex form. Moreover, he mentions the fact that extremely short period disturbances have been observed which are of large amplitude and of such short time interval that their form cannot be determined. When it is remembered that the cathode ray oscillograph does not respond accurately for frequencies much in excess of 50,000 it will be seen that later information points to the existence of violent atmospheric disturbances of extremely short duration. These disturbances would undoubtedly account for the observed effects in wireless receiving circuits, and the fact that 87 per cent. of these atmospherics have been traced to meteorological disturbances obviates the necessity for searching for some source of interference outside the atmosphere.

Dr. J. **Robinson** said that very little had been heard that evening about the effect of the earth's conductivity, yet the importance of this was shown, for instance, by the experiment of Dr. Smith-Rose quoted by Dr. Simpson on the passage of wireless signals over land and over sea. A type of propagation formula different from those hitherto considered would apply to signals sent from one aeroplane to another. It is known that transmission between 'planes is better than transmission between ground stations, and, further, that with a wave-length greater than the natural wave-length, end-on propagation occurs. Facts like these appear to demand experimental investigation.

Mr. C. E. P. **Brooks** (communicated): The distribution of thunderstorm frequency has recently been investigated in the Meteorological Office and a Geophysical Memoir embodying the results is now in the press. It was found that the average number of thunderstorms a year per station over the whole world was sixteen. It was assumed that each station will record those thunderstorms which develop in or enter a circle of area 200 square miles surrounding the station, so that the earth experiences about sixteen million thunderstorms a year, or 44,000 a day. Estimating the average duration of a thunderstorm as an hour, this means that there will be in progress at any one moment about 1,800 thunderstorms in different parts of the world, The frequency of lightning flashes during a storm was taken as 200 per hour, giving a total of 360,000 flashes an hour, or an average of 100 per second.

Within a circular region with a radius of 1,000 km., centred at London, the average frequency of thunderstorms is about 11,500 during the six months October to March, and 56,500 during the six months April to June. Presumably, the average storm in these latitudes is less severe than the average tropical storm ; assuming a frequency of 25 lightning flashes per storm in winter and 50 in summer, the number of flashes is about one a minute in winter and eleven a minute in summer. The winter flashes are almost independent of the time of day, the summer flashes are most frequent in the afternoon.

Mr. Watson Watt's charts showing the seasonal distribution of apparent sources of atmospherics fit in very well with the seasonal variation of thunderstorm frequency. In winter thunderstorms are relatively more frequent near the Atlantic coast of Europe, and the occasional storms of Iceland and the Faroes are practically limited to this season. Winter thunderstorms are rare in the interior of Europe. In summer the frequency is greatest in the interior, and thunderstorms are rare on the Atlantic coasts of the British Isles. There is an important maximum of thunderstorm activity on the northern side of the Alps, which appears to be indicated on the summer and autumn charts of atmospherics.

Mr. F. E. SMITH, C.B.E., F.R.S. (Chairman), expressed the thanks of the meeting to those who had taken part in the discussion, and also the hope that the two Societies might hold a further joint meeting in the future.

REPLIES TO THE GENERAL DISCUSSION.

Dr. W. H. **Eccles**, in closing the discussion at the Meeting, said :—

The discussion has shown that the subject of atmospheric ionization is so very wide that there is difficulty in grasping all the facts and seeing their connexion with one another.

The broad questions left unanswered are : How many conducting layers are there in the atmosphere ? What is the position of each of these layers ? Which of them function in the propagation of wireless waves ? In which of them reside the sources of the strays or sturbs that trouble the wireless telegraphist ?

So far as radio telegraphy is concerned, Prof. Appleton and Dr. Simpson have cited observations which prove the great influence of land and sea and of the air above them. That the air has an influence might be expected from the data of atmospheric conductivity discussed by Dr. Chree. It is proved also by the observed connexion between weather and signals. For instance, tropical storms cause variations of signal strength ; the variations resemble those produced by solar eclipses, in which the darkening of the air improves transmission. It seems that the troposphere is the region of the atmosphere concerned in these phenomena, but perhaps the turmoil due to a cyclone reaches into the stratosphere. The stratosphere, or, as it is sometimes called, the isothermal layer, has, rather unexpectedly, escaped attention, unless Prof. Chapman's estimate of the height of the seat of the magnetic variations is not beyond the isothermal layer. Here the question arises : Is the terrestrial magnetician's ionized layer the same as the wireless man's ? We are still undecided. As a fact, the wireless man and the magnetician each want at least two conducting layers. Wireless phenomena can scarcely be understood at all without the aid of, firstly, a world-wide permanently ionized layer, and secondly a lower layer subject to daily ionization ; magnetic variations cannot be explained without the aid of a world-wide layer ionized daily together with a layer permanently ionized and mainly confined to the polar regions. The disparity between our requirements is at first sight regrettable, but it will doubtless lead to interesting hypotheses and discoveries.

The discussion did not bring out any information about the effects of auroral displays and magnetic storms on wireless propagation, so I might recall that the British Association Committee analysed ten years ago a great many far Northern observations, and could not find any real connexion between auroræ and wireless transmission. Moreover, no connexion has anywhere been observed between magnetic storms and wireless. This seems to indicate that wireless waves do not penetrate into the auroral layer. Perhaps the bottom of this layer, at a height of about 100 kilometres, is the impenetrable quasi-reflecting ceiling named after Heaviside.

Time does not permit me to summarise the mass of valuable work performed by Mr. Watson Watt on atmospherics (some of it in collaboration with Prof. Appleton). But we must thank Mr. C. T. R. Wilson for his illuminating suggestion that thunderclouds may by their electrostatic field ionize the rarefied air beneath a high conducting layer. This may help to elucidate the fact that strays or sturbs are often produced without visible lightning, and that a thunderstorm between two stations frequently affects inter-communication.

Prof. E. V. **Appleton** (communicated) : In reply to Prof. Fortescue, I do not think it is necessary to assume a reflecting layer with a very sharp boundary for night-time conditions. For mathematical purposes it has had to be assumed that the Heaviside layer acts as a spherical shell with a sharp boundary and a conductivity of the order of that of sea-water. But for such a boundary long waves would be reflected better than short waves, and we know that the reverse is the truth. The correct explanation is, I think, the one given in the Paper. Ionic refraction takes place in a region in which the ionization increases with the height and in which the pressure is so low that the time between the collisions of an electron and the gas molecules is longer than the period of the wave. The lower atmosphere is devoid of excessive ionization due to the rapid recombination of the ions after the geometrical sunset.

Prof. Fortescue has discussed the experiments of Mr. Watson Watt and myself on atmospheric wave-forms from the standpoint of the wireless engineer, and suggests that there may be atmospheric disturbances of so short a period that the oscillograph will not respond accurately to them. I take it that he comes to this conclusion because atmospherics are still troublesome, at times, on wireless circuits of time period of the order of 1 micro-second. But I think it will be found that large pulses of duration of 25 micro-seconds are capable of producing all the effects on short-wave receivers with which we are familiar. The notion that the pulse must be of the same order of duration as the time period of the circuit before the disturbance is serious seems to have originated in an article by Mr. T. L. Eckersley, who considered mathematically the effect of a pulse on a wireless receiver. Eckersley took E_0a^2/ a^2+t^2 as the expression for the pulse, E being the electric force and t the time, and showed that the effect of such a pulse on a wireless receiver was negligible, except when the time period T of the receiver was $2\pi a$, i.e., of the same order as the duration of the pulse. But I do not think that the use of such a type of pulse is justified. Let us consider for a moment the analogy between the atmospheric problem and the white light problem of physical optics. We may consider the energy of a pulse as distributed in its energy-spectrum. If we proceed in this way it may be shown that various pulses differ enormously in their energy-spectra. For example, in the type of pulse considered by Mr. Eckersley the energy is confined to a narrow band of the range of commercial wavelengths, so that the result he got is not surprising. On the other hand, if we use $E_0e^{-\lambda t}\sin\lambda t$ as the expression for the pulse, we get a spectrum with energy distributed through a large range of wavelengths. It is interesting that this latter expression, which represents a very strongly damped discharge, gives an energy-spectrum closely resembling that deduced from the effects of atmospherics on systems of different wavelength, while the expression used by Eckersley gives no agreement. We therefore get further support for our belief that atmospherics originate in strongly damped discharges.

Thunderstorms have often been considered as a negligible source of atmospherics, but I have always maintained that lightning flashes and discharges from thunderclouds could produce all the effects observed. A careful watch should be kept for further evidence of the origin of atmospherics in shower clouds when the latter can be seen at the receiving station. My own observations on near thunderclouds and shower-clouds have been that there have been no atmospherics, except with the flashes. But many observations of this kind would have to be made before we could pit this against Mr. Watson Watt's conclusion that only a quarter of the atmospherics are due to thunderstorms. There seems little doubt that the thunderstorm mechanism is responsible for the separation of charges, which brings about the discharge.

Like Dr. Simpson, I cannot accept the mere correlation of atmospherics and rain. The radiation field of the atmospheric indicates a discharge, and one of no small magnitude.

With reference to the thundercloud origin of atmospherics, and Mr. Brooks's most valuable information on thunderstorm frequency, I may perhaps be permitted to quote my remarks of a year ago from the Proceedings of the Institution of Electrical Engineers (6th February, 1924).

" Some years ago (see " Year-book of Wireless Telegraphy and Telephony," 1921, p. 1114) I pointed out that Mr. C. T. R. Wilson's observations on the electric field of thunderclouds might be used in deducing the order of magnitude of the electromagnetic disturbance arising from lightning flashes. From these measurements we may deduce that a lightning flash is an aerial with an effective height only to be measured in kilometres and with a rapidly changing current of the order of 20,000 amperes. There seems no doubt that thundercloud discharges can be of sufficient magnitude to account for the radiation fields observed. It has often been argued that it is difficult to imagine sufficient lightning flashes on the earth's surface to account for the number of atmospherics observed, but I do not think that this is the case, as the following considerations show. It is known that the earth as a whole remains negatively charged in spite of the positive atmospheric current flowing into it, which is of sufficient magnitude to neutralize the charge in 10 minutes. The chief problem of atmospheric electricity is to determine how this negative charge is maintained. Mr. Wilson's theory is that the negative charge is replenished by the electricity from thunderclouds either in the form of lightning flashes or intense ionization currents. According to one version of this theory, we must regard the thunderstorm regions of the earth's surface as sending positive charges to the Heaviside layer and negative to the ground, either in the form of lightning flashes or ionization currents, and thus maintaining the Heaviside layer at a million volts positive with respect to the earth. In accounting for the maintenance of the earth's charge in this way it may be estimated that at least 1,000 thunderclouds are in action at any one time. I have tried to link up this theory of the thunderstorm origin of atmospherics with the directional observations of Hoyt-Taylor, Round, Watson Watt, and Schindelhauer on atmospherics, and it appears possible to get satisfactory correlation if we assume that the thundery regions are situated mainly on a wide equatorial belt (and in particular South Africa and South America), which acts as a permanent source of atmospherics. If we accept the theory the next point is to consider what general rules it gives as to the average direction of arrival of atmospherics at any point on the earth's surface, and what form the diurnal variation will take. It is a well-known fact that there is a correlation between local time and thunderstorm frequency. Thunderstorms occur most frequently during the afternoon, and thus at that time we may expect places having the same longitude as the point in question to act as sources of atmospherics. So far as this point is concerned, the source of atmospherics should follow the sun. Secondly, there is the question of the proximity of a particularly thundery district either on or off the equatorial belt (e.g., a land area as opposed to a sea area). This region will be a marked source of atmospherics when its local time is afternoon. And, thirdly, we have to consider the question of the ease of transmission of electric waves through the atmosphere between the point of origin and the point of observation, and the diurnal variation of the transmission factor. According to Mr. Wilson's theory, the earth should possess its maximum negative charge when the maximum number of thunderstorms are in action at any part of the day. Now Mauchly has shown that the charge on the earth reaches a maximum about 18 G.M.T., and if we examine Schindelhauer's results we find that the atmospheric disturbances coming from the south and south-west reach a maximum at about the same time. This points to the conclusion that South Africa and South America may be regarded as the centres of gravity of the earth's thunderstorm and atmospheric-producing regions."

Dr. **Chree** (communicated): As there has been a good deal of indefinite talk about conducting layers in the atmosphere, I would offer a few remarks on the subject. If, as seems reasonable, aurora is accepted as a direct evidence of high conductivity, then according to Störmer's observations, the "layer" of high conductivity extends at least sometimes to above the 600 km. level. The lower edge of some types of aurora is very clearly defined, and in such cases Störmer's measurements indicate from 90 to 110 km. as the lowest level. There may at times be aurora possibly of a more nebulous type at lower levels, but the more recent Norwegian height observations afford no confirmation of this. According to Störmer's observations, what we may look for during aurora is high conductivity from a height of several hundred kilometres, possibly from the top of the atmosphere down to about 100 km. The conducting layer during aurora may have a sharply defined lower edge, but it is certainly not a thin layer.

On the side of terrestrial magnetism, the association of magnetic disturbance with brilliant changes of aurora, the 11-year period in the amplitude of the regular diurnal variation, and the 27-day interval in magnetic storms, all point to the upper atmosphere as the locus of the electric currents to which the regular and irregular diurnal variations of the magnetic elements are ultimately due. But I do not think that any final conclusion can yet be drawn from the magnetic phenomena as to the exact height of the conducting layer or layers, or as to the existence of more than one layer.

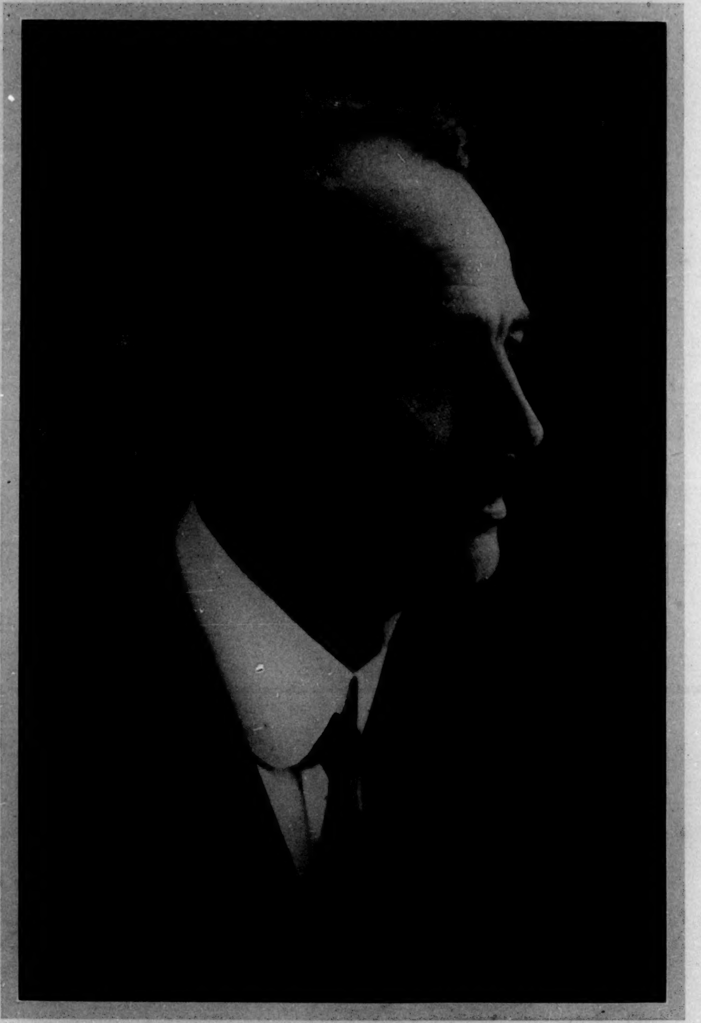
Prof. S. **Chapman** (communicated): With regard to Dr. Simpson's objection concerning the smallness of the pressure-differences associated with the lunar atmospheric tide, it must be remembered that these pressure-differences are not the cause of the tide, but are themselves, like the tidal motion, consequences of the gravitational tide-producing force. The latter is proportional to the mass of the element of air acted upon, and therefore tends to produce the same tidal acceleration of the air at high levels as at low levels.

I agree with Dr. Chree in doubting the appropriateness of the word "layer" for the auroral region of high conductivity, which may (in its most intense part) resemble a nearly vertical sheet rather than a flat layer.

Mr. R. A. **Watson Watt** (communicated): Prof. Fortescue's wide interpretation of my data as showing that we need not go outside the atmosphere in our search for the source of atmospherics is hardly justified by the data, which refer specifically to the selected cases in which short-wave direction-finding stations were able to observe a well-marked direction of arrival of atmospherics. Even so, only half the total number of cases where simultaneous bearings were obtained yielded locations within the wide area examined. I think Prof. Fortescue's view is correct, but it does not follow from the data under reference.

It is very satisfactory to have, from one with Mr. Brooks's encyclopædic knowledge of available data, a considered estimate of the world's supply of lightning, and those concerned with the study of atmospherics will await with interest the publication of the full memoir. A large mass of data on the distribution of atmospherics in time and in direction of arrival awaits correlation with such thunderstorm data.

I am glad to have Mr. Brooks's confirmation of my reading of the seasonal distribution charts, which I regarded as in close agreement with thunderstorm data.



Photograph by] [*Bassano.*

F. E SMITH
(PRESIDENT 1924-26

IX.—SYSTEMS OF ELECTRICAL MEASUREMENT.

By F. E. Smith, *C.B.E.*, *F.R.S.*

Introduction.

In the first place I wish to thank you for the honour you have done me in re-electing me as your President. For many years I have had the interests of the Society very much at heart, and I shall endeavour to serve it faithfully during the coming year.

Presidential addresses are in general of two types ; one a review of the history of the Society, and the other a review of some special branch of science. I have decided on the latter type and propose to review a branch of physics, the last chapter of which many regard as having been written. I refer to the C.G.S. System of Electrical Measurements. In my review I shall bear in mind the objects of the Society, more especially the "showing by new means of old facts" ; I shall endeavour to put before you certain ideals of Kelvin and Maxwell and other great physicists, who had the subject of units and standards very much at heart ; I shall try by means of experiments to show you that the C.G.S. System of Electrical Measurements is simple and not beyond the understanding of a good student of elementary physics ; my review will show that for the past 60 years there has been a continual struggle between a scientific ideal and a practical demand ; that struggle is still going on, and I propose to indicate a solution of the difficulty.

It is possible that the future will see a serious competitor of the C.G.S. system, for I believe it likely that there will arise a complete system of measurements based on the electron as the unit of electric quantity. Measurements of electric quantity in terms of the electron have been made for many years, but for practical purposes as visualised at present an electron system would not, I think, have any considerable advantages over the C.G.S. system.

I do not propose to review in detail the history of the C.G.S. system of electrical units. I must, however, emphasize certain features in the history, and in particular I shall concentrate on the following points.

(1) When the units based on the centimetre, gramme and second were recommended it was fully realised that the definitions and statements accompanying them were too complex to become popular definitions. In the first Report of the British Association Committee on Electrical Units and Standards, the following statement appears :—

> "It was apparent that, although a foot of copper or a metre of mercury might not be very scientific standards, they produced a perfectly definite idea in the minds of even ignorant men and might possibly, with certain precautions, be both permanent and reproducible, whereas Weber's unit has no material existence, but is rather an abstraction than an entity."

This statement is, I believe, almost equally true to-day and I shall endeavour this evening to overcome this objection to some extent by experiments showing how easy it is to measure resistance and current in terms of the centimetre, gramme and second.

(2) The second point I wish to draw attention to is the fact that we never have had standards identical with the units. In 1863 the Committee stated :—

"The practical adoption of the absolute system was felt to depend on the accuracy with which the absolute resistance of some one standard conductor could be measured."

And in another part of the Report the Committee state :—

"The only hesitation felt by the Committee was caused by doubts as to the degree of accuracy with which this admirable system could be or had been reduced to practice."

It is most important to note that unless a unit can be realised within limits of error not likely to affect industry there is no alternative to setting up a separate standard. A separate standard was set up in 1861, and the practice has continued until the present time. The B. A. Committee were greatly impressed by the advantages of the C.G.S. system, but they felt the units could not be realised sufficiently closely for practical purposes. In this they were correct. Their first attempt to realise the unit of resistance was in error by more than 1 per cent. The actual requirements of telegraph engineers at that time I do not know, but the comparative conducting powers of metals were being measured within 1 part in 10,000.

I shall emphasize the fact that the unit and standard have always differed.

(3) The third point is that not only have the unit and standard differed, but the standard has been changed several times, and another change will shortly be necessary. I shall endeavour to show that this change should be final and should result in the units and standards becoming practically identical.

EARLY ELECTRICAL SYSTEMS.

Before, however, proceeding with these main points, it is of interest to examine the nature of electrical measurements before the C.G.S. system was introduced. In the fifties of the nineteenth century, electrical engineering was practically confined to telegraphy; the transmission of signals by electromagnetic apparatus was suggested in 1821 by Ampere and shortly after this Gauss and Weber constructed a telegraph line over a distance of nearly two miles. In 1831, Henry produced a successful system for transmitting signals, but it was left to Morse in 1844 to establish the first long distance line, from Baltimore to Washington. Experiments on submarine telegraphy began in 1837, and the first Atlantic cable expedition began in 1857. In the eighteen-fifties it may be truly said that electrical engineering consisted of telegraphy. It is true there were dynamos, but it was not until 1866 that electromagnets were used in dynamo machines. It is not strange, therefore, that the names of some of the pioneers of the electric telegraph—Ampère, Gauss, Weber, and Henry—should be associated with electric units.

EARLY ELECTRICAL STANDARDS.

Sixty years ago resistances were expressed in terms of a length of telegraph wire, and voltages were given in terms of the number of batteries used. In England, the most commonly used standard of resistance was a mile of No. 16 gauge copper wire; in France it was a kilometre of iron wire 4 mm. in diameter; in Russia a copper wire 1 metre long and 1 mm. in diameter was used, and in Germany, Siemens' Unit, which was 1 metre length of mercury 1 sq. mm. in cross-section, was employed.

In addition to these Wheatstone proposed and used 1 ft. of copper wire weighing 100 grains. Resistance coils based on Wheatstone's standard were used by the Electric and International Telegraph Company. Two such standard coils are on the table ; these were made before 1861.

The following table shows some of the resistance standards previous to 1861 :—

Name.	*Nature.*
Jacobi	25 ft. of copper wire, weighing 345 grammes.
Siemens	1 metre of mercury, 1 sq. mm. cross section.
Digney	1,000 metres of iron wire, 4 mm. in diameter.
Matthiessen	1 mile of pure annealed copper wire, $\frac{1}{16}$in. diameter.
Varley	1 mile of a special copper wire, $\frac{1}{16}$in. diameter.
Wheatstone	1 ft. of copper wire, weighing 100 grains.
German Mile	8,238 yds. of iron wire, $\frac{1}{8}$ in. in diameter.

With regard to standards of voltage there were three, the Daniell Cell, which was invented in 1836, the Grove, invented in 1839, and the Bunsen, which was described in 1841. Voltages were usually expressed as a number of Daniell cells. There were also batteries of a simple type of which Davy in one experiment used 2,000.

There were no ammeters, the only current-measuring devices being galvanometers, and results were either recorded as deflections of these, or the wire and batteries used were stated. For submarine telegraphy measurements, a standard of capacity was needed ; in some cases a sea mile of Persian Gulf cable was the reference standard, and in other cases a sea mile of Atlantic cable. Such was the position in 1861.

British Association Committee.

It will be realized that in 1861, when the British Association appointed a Committee to improve the "Construction of Practical Standards for Electrical Measurements," the position can only be described as chaotic. Lord Kelvin served on this Committee for 34 years, Maxwell for 7 years, Lord Rayleigh for 27 years, and there were many other distinguished men, some of whom fortunately are still with us. The great result of the labours of this Committee was the adoption by the civilized world of a system of electrical measurements based on the centimetre, gramme, and second as the fundamental units ; it is known as the C.G.S. system, or absolute system. In their Report of 1863, the Committee state :—

"The absolute system is, however, not only the best practical system, but it is the only rational system. Everyone will readily perceive the absurdity of attempting to teach geometry with a unit of capacity so defined that the contents of a cube should be 6¼ times the arithmetical cube of one side, or with a unit of surface of such dimensions that the surface of a rectangle would be equal to 0·000023 times the product of its sides ; but geometry so taught would not be one whit more absurd than the science of electricity would become unless the absolute system of units were adopted."

The C.G.S. System.

In the C.G.S. system of electrical measurements the fundamental units are the centimetre, gramme and second, but as is now generally recognised, there is nothing in their nature which entitles them to be termed "absolute." In passing, I wish to point out that some alternative standard to the present international metre—a rod of platinum-iridium—is needed, and it is gratifying to note that at the last meeting of the International Committee on Weights and Measures (Metric) a resolution was adopted accepting in principle the eventual adoption of the wavelength of light as the ultimate reference standard of length.

What was it that led to the first proposal of a system of measurements based on three fundamental units? It appears that Gauss was engaged in measuring magnetic force and realising that all forces may be measured by the motions they produce, it was apparent that all forces could be measured in the same units, viz. a unit of length, of mass, and of time. Gauss therefore proposed a system of magnetic measurements based on these units. His colleague Weber in 1851 extended the idea, and proposed a general system of electromagnetic measurements based on the same three fundamental units. Lord Kelvin was greatly interested in the proposals which had been made, and extended them, and it was mainly due to him that the British Association Committee was appointed.

In electrical measurements we are concerned with current, resistance, voltage, quantity, force, work and so on. The real nature of many of these quantities is unknown, but Gauss found "force" to serve as a link between mechanics and magnetism, and Kelvin pointed out that "work" serves as a link connecting all branches of measurement in physical science. The dimensions of force are MLT^{-2}, and of work ML^2T^{-2}.

In electricity it is known that if two currents flow in two wires a force is set up which tends to move the wires. The common example is that of two coils through which the same current flows; a force of attraction or repulsion is set up. Hence the dimensions of current are $M^{\frac{1}{2}}L^{\frac{1}{2}}T^{-1}$.*

Again, we know that if a current flows through a resistance work is done, and the work done is proportional to the square of the current, the resistance and the time. Hence Work$=C^2Rt$, and the dimensional equation must be

$$ML^2T^{-2}=MLT^{-2}\times\frac{L}{T}\times T.$$

That is, the dimensions of resistance are the same as those of velocity. To measure a resistance it is therefore only necessary to measure a particular length and a particular time. Kelvin illustrated resistance being measured as a velocity in a manner somewhat as follows:—

RESISTANCE AS A VELOCITY.

Suppose a wire of any material to be bent into an arc of one radian, the radius being r cm. If a current i flows through this arc, the intensity of the magnetic field at the centre of the arc is (by definition of the unit of current)

$$\frac{i}{r}.$$

Let the arc of radius r be in the plane of the magnetic meridian with the upper extremity vertically over the lower one, and let two parallel wires or a continuous chain r centimetres apart join the ends as shown in Fig. 1. (The plane of the paper is supposed to be at right angles to the magnetic meridian.)

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* This assumes that the permeability μ of the medium has no dimensions. This is for convenience, but really the dimensions of μ are unknown.

Let the extreme ends be joined by a metallic rod L, and PP' be two rollers which when pulled in the direction X cause L to travel with a velocity V. Then the rate of cutting the lines of the magnetic field is HVr, and the current in the circuit is $\frac{HVr}{R}$, where R is the resistance of the circuit. Let the velocity be altered until the intensity of the field produced by the current in the arc is equal to H—i.e., a magnetic needle suspended at the centre of the arc is deflected 45°. Then, since

$$H=\frac{i}{r}$$

and

$$i=\frac{HVr}{R}$$

$$V=R$$

In other words, the velocity of the rod in centimetres per second is equal to the resistance of the circuit.

This experiment is not a convenient one to make, but an adaptation is easy, Such an adaptation is the well-known B.A. rotating coil devised by Kelvin. The principle of this is very simple, and if the self-inductance is neglected the calculation is easy for students.

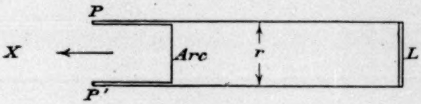

FIG. 1.

Consider a circle of wire spinning about a vertical axis in the earth's field with a magnet suspended at its centre. If A is the area of the circle, the average electromotive force is $\frac{4AH}{T}$, where T is the time of one revolution. The maximum E.M.F. is $\frac{\pi}{2}$ times this, i.e., $\frac{2\pi AH}{T}$, and the instantaneous E.M.F. in any position is

$$\frac{2\pi AH\sin\theta}{T}$$

$(90°-\theta)$ being the angle between the plane of the coil and the meridian.

The instantaneous current is

$$\frac{2\pi\, AH\sin\theta}{RT}$$

where R is the resistance of the ring. This current produces a magnetic field at the centre having a component at right angles to the meridian equal to

$$\frac{2\pi}{r}\cdot\frac{2\pi AH\sin^2\theta}{RT}$$

The average value of this for a complete cycle is

$$\frac{2\pi^2 AH}{r\,RT}$$

Hence the needle will be deflected through an angle φ such that

$$\tan\varphi = \frac{2\pi^2 A}{rRT}$$

and since

$$A = \pi r^2$$

$$R = \frac{2\pi^3 r}{T\tan\varphi}$$

It was not possible for the B.A. Committee to use a single turn, for a single turn is of low resistance, and accurate methods of comparing a very low resistance with others of medium value were unknown then. Later Lord Kelvin invented the double bridge for such comparisons. However, to-day, in good electrical laboratories, it is comparatively easy to compare a resistance of nominal value 0·001 ohm with another of 1 ohm, and in standardising laboratories such a comparison can be made with an error not greater than 1 part in 1,000,000.

ABSOLUTE MEASUREMENTS OF R.

I stated that one of my objects was to show by means of experiment that it is not difficult to measure to a first approximation an electric quantity in terms of C.G.S. units. I propose to demonstrate two methods for the measurement of resistance, one the method of a rotating coil or ring, and the other a mutual inductance method.

In spirit let us imagine we are back with Kelvin and the others in 1862. The practical units are fixed; it remains to see if we can realise our ideals.

I proceed to measure the resistance of this ring by the B.A. method of a rotating coil.

The mean radius r is 12·2 cm.
$2\pi^3 r$ is 756 cm.
T and φ by observation we find to be 0·1 second and 2·4 degrees respectively.
Hence the resistance R is $1{\cdot}8 \times 10^5$ cm. per second, or 0·00018 ohm.

The principle of the experiment is simple; it should be understood by any electrical engineer, for it is the simplest form of rotating armature.

In subsequent absolute measurements determinations were made by methods involving a mutual inductance. Kirchoff, Rowland, Glazebrook, Roiti and Himstedt and others used mutual inductances in their experiments. I propose to make a second measurement of resistance using a mutual inductance method that is very simple.

From the definition of unit current we know that the intensity I of the magnetic field at the centre of a circular coil is

$$I = Ci$$

C being equal to $\frac{2\pi n_1}{r_1}$ (the coil constant).

r_1 is the radius of the coil, n_1 the number of turns, and i is the current.

If a much smaller coil of effective area A ($A=\pi r_2^2 n_2$, where n_2 is the number of turns) is placed co-planar and co-axial with the large coil, and if a fluxmeter or ballistic galvanometer is placed in series with the small coil, the quantity Q_1 of electricity passing through the circuit containing the small coil when the current through the larger one is made or broken is approximately

$$Q_1=\frac{CAi}{R} \quad \text{. . . . .} \quad (1)$$

(the assumption is made that the field is uniform over the cross-section of the smaller, coil, an assumption only approximately true if the second coil is much the smaller one). In the example which follows the error is about 1 per cent. If in a second experiment the circuit of the larger coil includes the fluxmeter and is made for a short time t, the quantity Q_2 of electricity passing through the fluxmeter is

$$Q_2=it \quad \text{. . . . .} \quad (2)$$

Combining (1) and (2) we have

$$R=\frac{Q_2}{Q_1} \cdot \frac{CA}{t}$$

By adjusting t we may make $Q_1=Q_2$, and so eliminate any error due to calibration of the fluxmeter.

R is in absolute measure if the radii of the coils are in centimetres and t is in seconds.

In the apparatus before you

$r_1=36{\cdot}0$ cm.
$n_1=200$
$r_2=6{\cdot}0$ cm.
$n_2=200$

$C=34{\cdot}9$, $A=22{,}600$ and $CA=790{,}000$ cm.

With this apparatus, if Q_2 is made approximately equal to Q_1, the time t is of the order of 2×10^{-5} second. It is exceedingly difficult to measure such a short interval of time with accuracy, but it is not really necessary if a known fractional part of the current is used in the second experiment.

Fig. 2 shows the circuit I am using. In the circuit of the larger coil two resistances in parallel are included: the ratio of these resistances is 1/100,000 (their actual values need not be known), and in the second experiment the fluxmeter F is included in the high resistance branch of the circuit for a time t. Through this branch the current is $i/100{,}000$. Equation (2) becomes therefore

$$Q_2=\frac{it}{100{,}000}$$

and

$$R=\frac{Q_2}{Q_1} \cdot \frac{CA}{t} \cdot 10^5$$

In practice a fine fuse wire W_1 is included in the branch circuit, and a second fuse wire W_2 effectively shunts the fluxmeter. A simple pendulum with a heavy bob carrying a strong projecting wire is set swinging, the period being t. As it swings it is caused to break the fuse wire W_2, thus causing the current $i\times10^{-5}$ to flow through the fluxmeter. During the next swing it breaks the fuse wire W_1, thus causing the current through the fluxmeter to cease. The arrangement is such that the interval is exactly t, the period of the pendulum.

The period of the pendulum before you is 2·3 seconds. Q_2/Q_1 is measured by the fluxmeter to be 1·28 ; hence

$$R = 1{\cdot}28 \times 790{,}000 \times \frac{10^5}{2{\cdot}3} \text{ cm. sec.}^{-1}$$

$$= 4{\cdot}4 \times 10^{10} \text{ cm. sec.}^{-1}$$

$$= 44 \text{ ohms } (10^9 \text{ C.G.S. units}).$$

I hope these two experiments suffice to show that it is not difficult to make absolute measurements of resistance with simple apparatus, and with formulæ involving little more than a knowledge of the definition of the units.

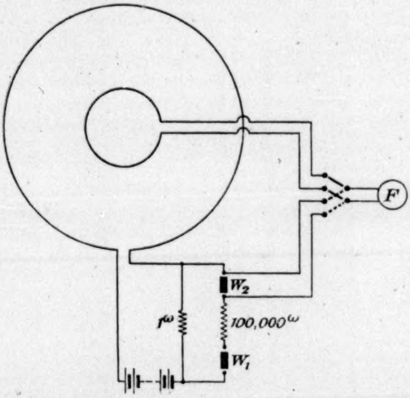

Fig. 2.

Unit and Standard.

I now propose to deal with my second principal point—namely, that we never have had standards identical with the units.

On the first page of their first Report the Committee drew an important distinction between a unit and a standard. It is necessary to realise that a unit need not have, and many units have not, material existence. However, in choosing their units the B.A. Committee specified that they should be perfectly definite, and should not be liable to require correction or alteration from time to time. The C.G.S. units comply with this specification, and no change has been made in them. The *standard* of resistance was, however, regarded as a material thing, and this also was not to be subjected to correction from time to time. It has, however, been subject to several changes, and it is desirable to consider the effect of this.

In the case of the International Standard and Unit of length the metre may be regarded as having been fixed first and the scientific unit—the centimetre—afterwards. But it is obvious that if the standard of length changes due to changes in the substance of which it is made the unit changes in the same proportion. The same is true for the international standard and unit of mass. These standards and units must be regarded as material things. Distinctly different is the unit of time, which is based on the rotation of the earth. The scientific unit of time—i.e., the interval in which the earth rotates through 1/86,400 part of the angle which it turns through in a mean solar day—cannot be represented by any material thing. The standard second corresponds to this unit, but for practical purposes standard clocks are used. If the second obtained through the medium of a standard clock changes, the unit is unaffected. In practice difficulties do not arise, because by continued astronomical observations the standard is made to correspond to the unit within limits quite negligible for all practical purposes.

As a general statement, therefore, it appears that when the standard and unit are material things, standard and unit change together; but when the unit is not a material thing any instrument constructed to realise it may change, and if the instrument is regarded as a standard, difficulties may arise unless continual checks are made. It is because the fundamental electrical units are analogous to the second, inasmuch as they are not material things, and also because for many years it was not possible to make the standards correspond to the units within limits negligible for all practical purposes, that it has been necessary to have numerous conferences to consider electrical units and standards.

The fundamental C.G.S. electrical units can never be material things, because time enters into the dimensions of all of them. Those of resistance are LT^{-1}, of current $L^{\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$ and of electromotive force $L^{3/2}M^{\frac{1}{2}}T^{-2}$. The B.A. Committee chose as practical units

For Resistance 10^9 absolute units
,, Voltage 10^8 ,, ,,
,, Current 10^{-1} ,, ,,

UNIT AND STANDARD OF RESISTANCE.

After devising apparatus to realise the unit of resistance, and after deciding on the best form and material for the standard to represent the unit,* there were two courses open to the Committee—

(1) To fix the limits within which the standard represented the unit and to recommend more experiments with a view to reducing the difference between the standard and the centimetre, second basis. As the limits of error were reduced the value of the standard would be expressed in an increasing number of significant figures.

(2) To regard the first determination as sufficient for all practical purposes for a long period, the standard being the one first constructed whether or not it agreed with the centimetre, second basis.

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* By the unit is meant 10^9 abs. units for resistance and 10^{-1} abs. units for current.

We may *imagine* a sequence of determinations as shown in the following table:—

Year in which absolute measurements were made.	Probable error of the absolute measurements.	Value in C.G.S. units of a material standard.	Errors permissible in industrial measurements. (a)	(b)
1861	±1 per cent.	0·99	±0·1 per cent.	±1·0 per cent.
71	±0·5	0·98$_5$	±0·1	±1·0
81	±0·1	0·988	±0·1	±1·0
91	±0·05	0·988$_5$	±0·01	±0·1
1901	±0·01	0·9887	±0·01	±0·1
11	±0·005	0·9887$_5$	±0·001	±0·01
21	±0·001	0·98873	±0·001	±0·01

Consider the 1861 figures. The difficulty of realising the C.G.S. unit at that time was such that subsequent measurements might alter the value by 1 per cent., whereas industry would be affected by a change of value of one-tenth of this amount.

Suppose that at the present time the nominal value of the standard of resistance was changed by 1 per cent. Then in testing electric lamps for candle-power the watts supplied would be changed by 1 per cent. and the candle-power per new watt would be changed by about 10 per cent. Such a change of nominal value of the standard would clearly be a grave matter. On the other hand, if the nominal value of the standard of resistance were changed by 0·05 per cent. the candle-power per new watt would be different by about 0·5 per cent. and might be a negligible amount to industry.

In general if the industrial position is that shown in column 4 the standard to be used for industrial purposes will possibly be appreciably different from the C.G.S. unit, for it will not be easy to change the nominal value of the standard. On the other hand, if the industrial position is that shown in the last column of the table, the nominal value of the standard may be changed so as to accord with the results of measurements of smaller probable error than the preceding ones.

The real position in 1861 is not clear to me, but the Committee decided, no doubt wisely, that the standard was not to be subject to correction from time to time, even though it differed appreciably from the unit. The Committee in their first report state :—

"Few defects could be more prejudicial than this continual shifting of the standard. This objection would not be avoided even by a determination made with greater accuracy than is expected at present, and was considered fatal to the unqualified adoption of the absolute unit as the standard of resistance."

It was agreed, however, that the material standard should be such as should ensure the most absolute permanency and should represent 10^9 cm./sec. as nearly as possible, and with the advance of science the difference between this standard and the 10^9 cm./sec. basis should be ascertained with increased accuracy in order that corrections could be applied by those who required them, but the material standard was under no circumstances to be altered in substance or definition.

In 1864 wire standards were made and issued, but it was proposed that the values of these should not be called "absolute measure," but that the name 1862 unit or B.A. unit, or "Ohmad," should be adopted.

From the commencement, therefore, it was realised that the unit and the standard might differ by an amount significant for practical purposes, and this position is true to-day. In 1861 the unit and the standard differed by 1 per cent., to-day the unit and standard differ by 0·05 per cent. Because of a difference between the C.G.S. unit and the standard many absolute measurements have been carried out since 1861. I do not propose to describe these, but the following list is of interest.

Date.	Observer.	Ohm in cm. of mercury.
	Method of Rotating Coil.	
1863	**B. A. Committee**	...
1881	**Rayleigh and Schuster**	...
1882	**Rayleigh**	...
1882	**H. Weber**	...
	Mutual Inductance Method.	
1878	Rowland	...
1883	Glazebrook	...
1884	Rowland	106·34
1884	Roiti	105·896
1886	Himstedt	106·08
	Weber's Method of Transient Currents.	
1885	Wiedemann	106·162
1885	Mascart, de Nerville and Benoit	106·37
	Weber's Method of Damping.	
1884	Wild	106·027
1888	F. Kohlrausch	106·34
1889	Dorn	106·243
	Method of Lorenz.	
1873	Lorenz	...
1883	Rayleigh and Sidgwick	...
1884	Rowland, Kimball and Duncan	106·29
1889	Duncan, Wilkes and Hutchinson	...
1891	V. Jones	106·307
1913	Smith	106·245
	Method of Alternating Current.	
1912	Campbell	106·273
1924	Campbell	106·243
	Gruneisen's Method.	
1920	Gruneisen and Giebe	106·246

UNIT AND STANDARD OF CURRENT.

I now turn to the second fundamental unit, the Ampère. In this case no material standard is possible, but nevertheless much the same procedure has been adopted, and a standard current has been specified which either deposits silver at a certain rate when passed through a silver voltameter or causes a specified force to be exerted between two coils forming part of a balance.

ABSOLUTE MEASUREMENT OF CURRENT.

The absolute measurement of electric currents by their mutual action on one another was explained very simply by Maxwell in 1863. Maxwell's two coils were a long way apart, and their planes at right angles to each other. Weber suggested two coils, a large and a smaller one suspended centrally, the axes of the two coils being inclined. The first current weigher or current balance was used by Joule, and consisted of two flat co-axial coils.

The coils described by Maxwell are the equivalent of magnetic shells, and Maxwell's description led me to build a small coil—somewhat like a modern wireless inductance—with a comparatively small moment of inertia, but capable of carrying a large current. The coil I am about to experiment with is the same I used in the mutual inductance method for the measurement of resistance; it weighs 80·5 grammes, has an effective radius of 6·0 cm., consists of 200 turns of enamelled wire, and a current of 2 ampères can be passed through it without undue heating. This coil is equivalent to a magnetic shell of moment Ai, where i is the current and A is the total effective area ($n\pi r^2=22{,}600$). The moment of inertia of the system is 1,450 C.G.S. units.

This small coil is set swinging at the centre of a large coil carrying the same current i; the time of swing of the small coil about a vertical axis is

$$t=2\pi\sqrt{\frac{I}{AiF}}$$

where I is the moment of inertia of the small coil, and F is the intensity of the field at the centre. F is equal to Ci, where C is the coil constant ($2\pi n/r$). Hence

$$t=2\pi\sqrt{\frac{I}{ACi^2}}$$

that is,

$$i=\frac{2\pi}{t}\sqrt{\frac{I}{AC}}$$

I propose to use the same large coil as I used in the measurement of resistance. The plane of the coil is in the magnetic meridian. For this coil $C=34{\cdot}9$.

Hence—

$$i=\frac{2\pi}{t}\sqrt{\frac{1{,}450}{22{,}600\times 34{\cdot}9}}$$

$$=\frac{0{\cdot}269}{t}\text{ C.G.S. unit.}$$

I proceed to make an experiment. The time of swing only has to be determined. t is found to be 1·9 seconds. Hence

$$i=0{\cdot}142\text{ C.G.S. unit.}$$
$$=1{\cdot}42\text{ amperes }(10^{-1}\text{ C.G.S.}).$$

A correction for the intensity H of the earth's horizontal magnetic field may be made by measuring the change in the position of rest of the suspended coil when the current through both coils is reversed. If 2θ is the change in the angular position, $\tan\theta=H/F$ and the resultant field acting on the suspended coil is $F/\cos\theta$. The corrected formula for the current is

$$i=\frac{2\pi}{t}\sqrt{\frac{I\cos\theta}{AC}}$$

In the experiment just made the correction is about 4 parts in 10,000.

CURRENT WEIGHER.

By far the most direct and instructive way of measuring current is to measure the force between two coils carrying the current and various forms of current weighers have been devised to do this.

The form of current weigher I propose to experiment with is simple. It consists of a large coil of many turns with a smaller coil supported at its centre, the planes of the two coils being at right angles, and the smaller being free to swing like the beam of a balance, the axis of motion being in the plane of the large coil. It is similar in principle to Pellat's current weigher.

If a current i flows through both coils the intensity I of the field at the centre of the large coil is

$$Ci,$$

where C is the coil constant as before.

The small coil is equivalent to a magnetic shell of moment

$$Ai,$$

A being the effective area as before, and the couple acting on the coil is

$$ACi^2.$$

This is balanced by a mass m at distance l from the centre. Hence

$$mgl = ACi^2,$$

or

$$i^2 = \frac{mgl}{AC}$$

In the balance I have constructed the knife "edges" consist of wires of the cobalt steel known as "stellite." The "planes" of the balance are also of stellite, and the current passes into and out of the coil by means of these wires and planes. The large coil is the one I have already used with constant C ($C = 2\pi n/r$), equal to 34·9; this is the intensity of the field at the centre of the coil when unit current flows through the coil.

The small coil is 4·7 cm. in effective radius and has 200 turns. It is equivalent to a magnetic shell having a moment A ($A = \pi r^2 n$) of 13,900 C.G.S. units.

The product AC is $48{\cdot}4 \times 10^1$ C.G.S. units.

The length l of an arm of the balance is 5 cm., and $g = 981$ cm./sec.^{2}.

Hence

$$i^2 = \frac{m \times 981 \times 5}{484{,}000}$$

or

$$i = \frac{\sqrt{1{\cdot}01m}}{10} \text{ C.G.S. units.}$$

In the experiment I choose $m = 1$ gramme, and adjust the current through the coils until the beam is balanced. The current is then $0{\cdot}100_5$ C.G.S. unit or $1{\cdot}00_5$ ampere.

STANDARDS OF CURRENT.

Current has been and is frequently measured in C.G.S. units through the tangent and sine galvanometers, the horizontal intensity of the earth's magnetism being measured by the method of Gauss. There are no material standards of current, but a current weigher has been erected by the Board of Trade as a standard, and the silver voltameter has been specified as a standard instrument. A combination of a standard cell with a standard resistance also serves as a standard of current. Any variations in a standard current weigher such as that of the Board of Trade arises because of changes in the dimensions of parts of the balance. Any variations in a standard such as a silver voltameter occur because of our lack of knowledge of the actions which take place, or they may be due to changes of procedure. Such variations can be made very small.

The relation between the C.G.S. unit of current and certain standards has been measured by many experimenters; the majority of these results are given in the following table:—

RESULTS OF ABSOLUTE MEASUREMENTS OF CURRENT.

Date.	Observer.	Results given by Author.		
		Electro-chemical equivalent of silver in mgm.	E.M.F. of Clark cell.	E.M.F. of Weston Normal cell at 20°·0 C.
	1. *Method of Tangent Galvanometer.*			
1873	F. Kohlrausch	1·1363	......	...
1886	F. and W. Kohlrausch...	1·11833	......	...
1904	Van Dijk and Kunst ...	1·1180	......	...
1910	Haga and Boerema ...	1·11802	......	1·01826*
	2. *Method of Sine Galvanometer.*			
1872	Latimer Clark ...	......	1·4562† B.A. volts at 15°·5C.	...
1886	T. Gray	1·118	......	...
	3. *Electrodynamometer Methods.*			
1872	Latimer Clark ...	......	1·4573† B.A. volts at 15°·5C.	...
1898	Patterson and Guthe ...	1·1192	......	...
1899	Carhart and Guthe ...	......	1·4333* at 15°·0 C.... ...	...
1906	Guthe	1·11773	1·43296* at 15°·0 C. ...	1·01853*
1913	Shaw	......	......	1·01827*
	4. *Current Weigher Methods.*			
1882	Mascart...	1·1156	......	...
1884	Rayleigh	1·11794	1·435 at 15°·0 C.	...
1890	Pellat and Potier ...	1·1192	......	...
1908	Ayrton, Mather & Smith	1·11827	......	1·01818*
1908	Janet, Laporte and Jouast	1·11821	......	1·01836*
1908	Guillet	......	......	1·01812*
1908	Pellat	......	......	1·01831*
1910	Smith	1·11815	......	1·01818*
1911	Rosa, Dorsey, Miller ...	1·11804	......	1·01822*
1914	Shaw	......	......	1·01831*

Various forms of voltameters were used, and the numbers given for the electrochemical equivalent do not enable a good comparison to be made.

*** The E.M.F. is expressed in terms of the ampere (10^{-1} C.G.S.) and the international ohm.**

† The E.M.F. is expressed in terms of the ampere (10^{-1} C.G.S.) and the B.A. unit.

PROBABLE CHANGES.

My final point is that not only have the electrical standards been varied in the past because they have differed from the units, but another change must be made shortly.

At present the standard of resistance differs from the unit by about 5 parts in ten thousand; the standard of electromotive force differs from the unit by about the same amount, while the standard of current is in close agreement with the unit of current (10^{-1} C.G.S.).

The differences are too great. A large number of physical measurements depend on the fundamental electrical standards, and there are many reasons why our standards should be in closer agreement with the C.G.S. units.

In 1881 the B.A. Committee reported :—

"It might be well to reconsider the question whether the 'ohm' should be defined by reference to a particular coil of wire preserved as a concrete standard, or whether the term 'ohm' should be understood to mean a resistance of 10^9 C.G.S. units."

In 1881 this was not possible as the errors in realising the unit were too great. To-day the difficulties have been swept aside by the work of the great national laboratories of Great Britain, the United States, France, Germany and Japan, and I venture to suggest that the next change should be for the units and standards to be identical, the ohm to be 10^9 C.G.S. units and the ampere to be 10^{-1} C.G.S. unit, the limits of error to be stated. At present resistance and current can be measured in C.G.S. units within a few parts in 100,000, perhaps 1 part in 100,000. The international units cannot be realised any closer than this, and actually the work of realising the international units is the more expensive.

At present the National Physical Laboratory has apparatus for measuring resistance and current in C.G.S. units, and work is in progress to make the current balance more precise and easier to manipulate. The Bureau of Standards has a fine current balance, and for this I understand new coils are to be built. At the Bureau there are at present two methods being experimented with for the absolute measurement of resistance. Both of these methods make use of stationary coils, the mutual inductance or self-inductance of which can be computed from measured dimensions. Since the coils are stationary, the dimensions can readily be determined with great accuracy. Moreover, the effect of external magnetic fields will be negligible. In one method the drop in potential over a resistance is compared with the electromotive force induced in the secondary of the mutual inductance. This comparison is made directly without the use of a condenser. In the second method, the self-inductance is compared with a condenser, whose value has been determined in terms of the international ohm.

In France excellent work has been done on the absolute measurement of current, and at the Reichsanstalt apparatus exists for the absolute measurement of resistance.

In the next few years I believe the change will come, and the international standards will be displaced, for they must be regarded, in the words of Lord Rayleigh, as a "fifth wheel to the coach."

X.—AN INVESTIGATION INTO CORRECTIONS INVOLVED IN THE MEASUREMENT OF SMALL DIFFERENCES IN REFRACTIVE INDEX OF DISPERSIVE MEDIA BY MEANS OF THE RAYLEIGH INTERFEROMETER, WITH SPECIAL REFERENCE TO THE APPLICATION OF THE RESULTS TO MEASUREMENTS IN DIFFUSION.

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Received May 8, 1924.

ABSTRACT.

An account is given of the use of a Rayleigh interferometer in a series of subsidiary experiments conducted in connexion with a recent determination of the diffusivity of solutions of salts in water.

The theory of the instrument, as affected by the different dispersions of the various media traversed by the light-rays, is studied, and the observations are corrected so as to apply to the green mercury light $\lambda=5461$ Å, which was employed in the experiments on diffusion.

§ 1. INTRODUCTION.

IN the course of a recent investigation on Diffusion in Liquids by an optical method (Proc. Phys. Soc., v. 36, p. 313, 1924), it was necessary to know the value of the rate of variation ($d\mu/dn$) of the refractive index, for green mercury light $\lambda=5461$ Å, of a salt solution with respect to its concentration, for the different salts investigated in that research, and for various strengths of those solutions.

A considerable amount of work has been done on the connexion between the refractive index of an aqueous salt solution and its concentration, and it was at first anticipated that sufficient data would be found published to supply the information required.

It is, however, evident that to obtain ($d\mu/dn$) to an accuracy of, say, 1 per cent., a considerably higher accuracy is necessary in the values of μ, and on comparing the published results with one another, it was found that this degree of accuracy was often not realised in the researches carried out 20 or 30 years ago, and still quoted as standard results.

See Wagner (Tables for Dipping Refractometer, Sondershausen, 1907).
Bender (Wied. Ann., v. 39, p. 89, 1890), (Ann. d. Phys., v. 8, p. 109, 1892).
Schütt (Zeit. f. ph. Chem., v. 5, p. 349, 1890).
Dinkhauser (Wien. Ber., v. 114, p. 1001, 1905).
Borgesius (Wied. Ann., v. 54, p. 221, 1895).
Hallwachs (Wied. Ann., v. 47, p. 380, 1892).
Dijken (Ziet. f. ph. Chem., v. 24, p. 81, 1897).

A comparison of the results of these researches will show that, for the refractive index of solutions of the same salt, under similar conditions of concentration, temperature, &c., variations of a unit in the fourth decimal are not uncommon, and an error of this magnitude causes an error of about 2 per cent. in the differential ($d\mu/dn$).

Moreover, apparently no experimenter has employed the mercury arc as his luminous source in these investigations, and in most cases it is impossible to correct the published results so as to apply to $\lambda=5461$ Å, owing to insufficient observations having been made to permit the necessary interpolations.

It was discovered, too, that in some cases very few results were available for solutions of some of the salts whose diffusivity had been experimented upon (e.g., potassium nitrate).

In order to obtain the required results in these cases, and, even where sufficient data are otherwise available, to avoid the inaccuracies liable to be associated with interpolation for both temperature and wavelength, it was decided that it would be advisable to make a special determination of the quantity under consideration $(d\mu/dn)$, under conditions similar to those which hold in the diffusion experiments; and of the various methods of experiment at our disposal, it was resolved to employ an interferometer of the Rayleigh type (Proc. Roy. Soc., v. 59, p. 202, 1896).

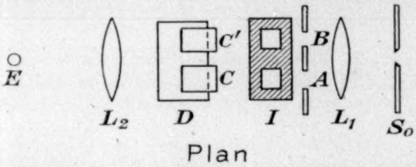

FIG. 1.

§ 2. APPARATUS.

The general arrangement of the apparatus is shown in Figs. 1 and 2, the source of light being a 100-volt, 60-watt, gas-filled electric lamp. Fiducial fringes, produced by the pencils of light which pass through the apertures *A* and *B* (about 3 mm. wide and 1 cm. apart) and the glass base of the interferometer cell *I* are seen in the lower part of the field of view of the eyepiece *E*, which, in accordance with Lord Rayleigh's suggestion, is made of a short piece of glass rod about 3 mm. diameter set with its axis vertical, in order to give the magnification necessary to observe the fringes due to such widely separated sources as *A*, *B*.

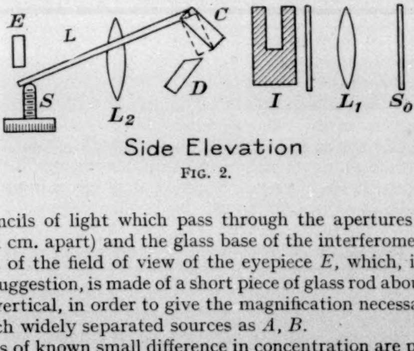

FIG. 2.

Two solutions of known small difference in concentration are placed respectively in the two parts of the double cell *I*, and are traversed by the two beams of light.

The resulting interference fringes are displaced with respect to the fiducial comparison fringes, and, in general, entirely disappear from view with the source of light employed, as the difference in the optical paths through the two solutions is too great.

The compensating plate C' (Figs. 1 and 2) was fixed at about 45° to the horizontal, while C was mounted on a horizontal axis, so as to be capable of rotation by means of a lever L and the screw S, of pitch 1/50th of an inch, the head of which was divided into 200 parts, so that its least count was 0·0001 in.

By rotating this screw, the effective thickness of the compensating plate C can be varied, and so the sum of the optical paths through the solution and the compensator may be made equal for the two beams passing through A and B respectively.

When this is the case, the interference fringes are seen to appear in the upper part of the field of view also, and the " achromatic " fringe can be adjusted so as to correspond exactly in position with the similar " achromatic " fringe seen in the lower part of the field.

The reading r of the screw S is then taken. The zero reading r_0 of the screw can similarly be found when the interferometer cell contains similar solutions in both parts, and $(r-r_0)$, the difference between the screw readings, enables us to calculate the difference $d\mu$ in the refractive indices of the two solutions under investigation, whose concentration-difference is dn.

§ 3. THEORY OF THE INTERFEROMETER.

The effect of dispersion in the solutions and the compensating plates is frequently neglected in using the interferometer to measure the refractive index of solutions and similar media, and the theory connected with this aspect of the problem seems to have been little studied.

L. H. Adams (Jour. Wash. Acad. Sci., v. 5, p. 265, 1915) has given some account of the theory involved, but he appears to make certain assumptions which can hardly be accepted completely. For instance, he says : " If (as in actual practice) the solution has shifted these fringes N fringes to the left, and the compensator an equal number to the right . . ." (loc. cit., p. 275).

This in general does not occur in actual practice, for the shorter optical path through the diluter solution is increased by altering the effective amount of glass in that path by a rotation of the compensator plate. The relative dispersions of the two solutions and of glass and air cause a " wandering " of the achromatic fringe, i.e., the achromatic fringe moves relatively to the interference bands as the plate is rotated, so that there is no guarantee that the plate has shifted the fringes by an exactly equal and opposite amount to the displacement caused by the solutions.

The treatment of the problem given by R. W. Wood (" Physical Optics ") and by C. R. Mann (" Manual of Advanced Optics ") refers to the Michelson interferometer, and is not easily applicable to the present instrument.

A more complete investigation of the theory would appear to be as follows :— Consider rays from two similar sources A and B (Fig. 3) at a distance a apart, falling on a screen OQ. Let rays AO and BO pass respectively through solutions of refractive indices μ_1 and μ_2 for light of a wavelength λ, and also through glass plates of refractive index μ inclined at an angle i to the rays, where i is in the neighbourhood of 45°.

The optical path difference introduced by the plate,

$$P=\mu AB-AC \text{ (see Fig. 4)}$$
$$=\mu e/\cos r-(e/\cos r)\cos(i-r),$$

which simplifies to

$$P=e[\mu\cos r-\cos i] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where e is the thickness of the compensating plate, and i and r are the angles of incidence and refraction respectively.

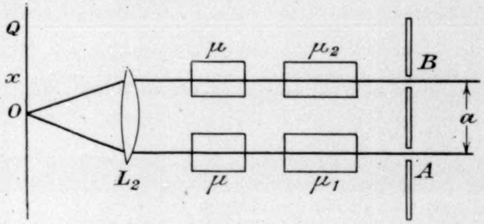

FIG. 3.

If $H=2\mu^2\ 1$,

$i=45°+\theta$, where θ is a small angle. Then

$$P=(e/\sqrt{2})\,[\sqrt{H-\sin 2\theta}-\cos\theta+\sin\theta] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Similarly, the optical path difference introduced by the solution is $E(\mu_1-1)$ and $E(\mu_2-1)$ respectively, where E is the length of the cell of the interferometer traversed by the rays.

If the same solution is in both parts of the interferometer cell and the compensating plates of equal thickness e are exactly parallel to one another, the phase of waves of wavelength λ from A (Fig. 3) lags behind that of waves from B by an amount $(2\pi/\lambda)(ax/f)$, where x is measured from the centre O of the screen towards Q, and f is the focal length of the lens L_2 (Figs. 1, 2 and 3).

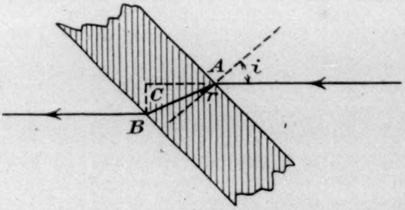

FIG. 4.

This phase difference is zero at the point where $x=O$ for all wavelengths, so that the central fringe at O is achromatic under the conditions mentioned ; but it is increased both by a difference in the concentration of the solutions in the cell and by a rotation of the compensating plates by amounts $(2\pi/\lambda)E(\mu_1-\mu_2)$ and $(2\pi/\lambda)(P_1-P_2)$, where the suffixes refer to the paths of waves from A and B respectively.

Hence the total phase difference between the waves from A and B at any point x on the screen OQ (Fig. 3) is given by

$$\varphi=\frac{2\pi}{\lambda}\left\{\frac{ax}{f}+E(\mu_1-\mu_2)+(P_1-P_2)\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (3)$$

Now at the *central fringe* of the displaced system—that is, the fringe of zero path difference, $\varphi=o$.

If x_0 denotes the position of this displaced central fringe,

$$x_0=(f/a)[E(\mu_2-\mu_1)-(P_1-P_2)], \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (4)$$

which gives a general expression for the position of the central fringe for any two solutions in the cell of refractive indices μ_1 and μ_2, and for any inclinations of the compensating plates suggested by the quantities P_1 and P_2.

It will be observed that before the plates are turned to produce compensation—i.e., while $P_1=P_2$—the displaced position of the central fringe (*see* Fig. 5), produced by the solutions alone, is given by—

DISPLACED CENTRAL FRINGE.

$$x'_0=(f/a)E(\mu_2-\mu_1). \quad . \; . \; . \; . \; . \; . \; . \; . \quad (4a)$$

These equations (4) and (4a) indicate a displacement of the central fringe which varies with the colour of the light employed, since the refractive indices vary with the wavelength, so that, using white light, the central fringe will be displaced by different amounts for the different colours and will in general be no longer achromatic. Indeed there will be no strictly achromatic fringe at all in the displaced fringe system, but an approximately achromatic line, which will not in general coincide with the central fringe, will be obtained at the position where the change in phase-difference with change in wavelength is zero.

That is, *for achromatism*, $d\varphi/d\lambda=o$. Therefore, from equation (3),

$$ax/f+E(\mu_1-\mu_2)+(P_1-P_2)=E\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)+\lambda d(P_1-P_2)/d\lambda.$$

If x_1 denotes the position of this " achromatic line " in the general case,

$$x_1=(f/a)[E(\mu_2-\mu_1)-(P_1-P_2)+E\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)+\lambda d(P_1-P_2)/d\lambda]. \quad . \; . \; . \quad (5)$$

The condition for achromatism will not in general be satisfied at a position which coincides with one of the maxima of the interference fringes observed along OQ (Fig. 3), for which the condition holds that φ is an integral number of times 2π, for the brightest component of the light used.

In experimental work, that particular fringe will be selected as most nearly achromatic, which is nearest to the theoretical achromatic line given by $d\varphi/d\lambda=o$.

The values of (P_1-P_2) and of $d(P_1-P_2)/d\lambda$ may be found by putting $\sin\theta=0$ and $\cos\theta=1$, in equation (2). Thus we find

$$(P_1-P_2)=(e/\sqrt{2H})(\sqrt{H}-1)(\theta_1-\theta_2)\,. \qquad (6)$$

$$d(P_1-P_2)/d\lambda=[2\mu(P_1-P_2)/H(\sqrt{H}-1)](d\mu/d\lambda). \qquad (7)$$

DISPLACED ACHROMATIC LINE.

Before compensation, $P_1=P_2$, and the displaced position of the " Achromatic Line " (*see* Fig. 5) is given by

$$x'_1=(f/a)[E(\mu_2-\mu_1)+E\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)] \qquad (5a)$$

A consideration of equations (4*a*) and (5*a*) will show that for the optical media employed in this investigation, the displacement x'_1 of the achromatic line produced by the solutions alone (i.e., with $P_1=P_2$) is greater than x'_0, that of the central fringe under the same conditions.

Now, suppose that the displacement of the fringes produced by the difference in concentration of the solutions is " compensated " by a rotation of one or both of the plates, so that the " achromatic line " is restored to its undisplaced position (*see* Fig. 5), and suppose that (p_1-p_2) is the special value of (P_1-P_2) required for this compensation. It is to be observed that in practice we cannot with certainty perform this operation, for the achromatic *line* may differ in position from the achromatic *fringe* by an amount not greater than half a fringe-width, and it is only the achromatic fringe which can be identified in experimental work.

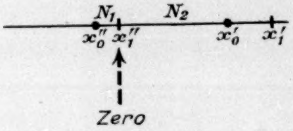

FIG. 5.

If, however, the compensation suggested could be exactly performed, then equation (5) becomes

ACHROMATIC LINE RESTORED.

$$x''_1=o=(f/a)[E(\mu_2-\mu_1)-(p_1-p_2)+E\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda) \\ +\lambda d(p_1-p_2)/d\lambda]$$

i.e., $$E(\mu_2-\mu_1)-(p_1-p_2)=-[E\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)+\lambda d(p_1-p_2)/d\lambda] \qquad (8)$$

And from equation (4),

SIMULTANEOUS POSITION OF CENTRAL FRINGE.

$$x''_0=(f/a)[E(\mu_2-\mu_1)-(p_1-p_2)] \qquad (4b)$$

$$=-(f/a)\{E\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)+\lambda d(p_1-p_2)/d\lambda] \qquad (9)$$

which gives the position of the central fringe at the time when the " achromatic line " has been restored to its original undisplaced position, by a rotation of the compensator plates.

An inspection of equation (9) will demonstrate that for ordinary kinds of glass, when the displacement of the achromatic line has been exactly compensated by a rotation of one of the plates, that of the central fringe is *over* compensated—i.e., the movement of the screw S (Fig. 2) has been in general greater than is necessary to restore the central fringe to its undisplaced position, and as x''_0 is found to be numerically less than $(x'_1-x'_0)$ (*see* Fig. 5), it follows that the dispersion in the glass introduced to effect the compensation partly counter-balances the dispersion of the solutions.

Again, in order to simplify these equations for practical application, suppose that it were practicable to turn the compensating plates so that the *central fringe* were undisplaced, and let (p_3-p_4) be the special value of (P_1-P_2) required for this compensation. Then equation (4) would become

$$o=(f/a)[E(\mu_2-\mu_1)-(p_3-p_4)] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4c)$$

$$\text{i.e., } E(\mu_2-\mu_1)=p_3-p_4. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

This adjustment of the plates, which would enable us readily to find $(\mu_2-\mu_1)$, is unfortunately not practicable, since we have no means of identifying the central fringe. The " achromatic " fringe is the only one which can be identified.

Then, from equations (9) and (4*a*), we have

$$\begin{aligned}-x''_0/x'_0&=[E(\lambda d\mu_1/d\lambda-d\mu_2/d\lambda)+\lambda d(p_1-p_2)/d\lambda]/E(\mu_2-\mu_1)\\ &=[1/(\mu_2-\mu_1)][\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)+(\lambda/E)\,.\,d(p_1-p_2)/d\lambda], \quad . \quad . \quad . \qquad (11)\\ &=[1/(\mu_2-\mu_1)][\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)]+2\mu/[H(\sqrt{H}-1)](\lambda d\mu/d\lambda)\\ &\qquad [(p_1-p_2)/(p_3-p_4)] \quad . \quad . \quad . \qquad (11a)\end{aligned}$$

from equations (7) and (10).

This gives the ratio of the over-compensated displacement $(-x''_0)$ of the central fringe when the " achromatic line " has been restored to its undisplaced position, to the total displacement (x'_0) of the central fringe produced by the solutions under investigation before compensation. Now, if N_1 denotes the number of fringe-widths for a wavelength λ through which the " achromatic line " has been shifted relatively to the central fringe, in the process of compensation just referred to, and N_2 denotes the total number of fringe-widths in the displacement of the central fringe by the solutions before any compensation has been effected, then

$$N_2\lambda=ax'_0/f=E(\mu_2-\mu_1), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

and $N_1/N_2=-x''_0/x'_0$ (*see* Fig. 5). Also,

$$\begin{aligned}\frac{p_1-p_2}{p_3-p_4}&=\frac{E(\mu_2-\mu_1)+E\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)+\lambda d(p_1-p_2)/d\lambda}{E(\mu_2-\mu_1)}\text{ from equations (8) and (10)}\\ &=1+[1/(\mu_2-\mu_1)][\lambda(d\mu_1/d\lambda-d\mu_2/d\lambda)+(\lambda/E)\,.\,d(p_1-p_2)/d\lambda]\\ &=1+N_1/N_2 \text{ from equation (11).}\end{aligned}$$

Substituting these values in equation (11*a*), we have

$$\frac{N_2}{N_1+N_2}=\left[1-\frac{2\mu}{H(\sqrt{H}-1)}\,.\,\lambda\frac{d\mu}{d\lambda}\right]\Big/\left[1+\frac{1}{\mu_2-\mu_1}\,.\,\lambda\,.\left(\frac{d\mu_1}{d\lambda}-\frac{d\mu_2}{d\lambda}\right)\right] \quad . \quad . \qquad (13)$$

Now let us assume

$$\begin{aligned}\mu&=A+B\lambda^{-2} &&\text{i.e., } \lambda d\mu/d\lambda=-2B/\lambda^2.\\ \mu_1&=A_1+B_1\lambda^{-2} &&\text{i.e., } \lambda d\mu_1/d\lambda=-2B_1/\lambda^2.\\ \mu_2&=A_2+B_2\lambda^{-2} &&\text{i.e., } \lambda d\mu_2/d\lambda=-2B_2/\lambda^2.\end{aligned}$$

Then, from equation (13)

$$\frac{N_2}{N_1+N_2}=\left[1+\frac{2}{\lambda^2}\cdot\frac{2\mu(\mu-1)}{H(\sqrt{H}-1)}\cdot\frac{B}{\mu-1}\right]\Big/\left[1+\frac{2}{\lambda^2}\cdot\frac{B_2-B_1}{\mu_2-\mu_1}\right] \quad . \quad . \quad . \qquad (14)$$

Now for the spectrum lines F and C, expressing λ in microns,

$$\mu_F-\mu_C=B(1/0\,.\,486^2-1/0\,.\,656^2)=1{\cdot}91B.$$

$$\therefore \qquad \frac{B}{\mu_D-1}=\frac{\mu_F-\mu_C}{\mu_D-1}\times\frac{1}{1{\cdot}91}=\frac{\bar{\omega}}{1{\cdot}91} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (15)$$

where $\bar{\omega}$ is the dispersive power of the compensator plate, and may be taken as 0·015 for crown glass.

Again, if we define β by the identity

$$\beta\equiv[(\mu_2-\mu_1)_F-(\mu_2-\mu_1)_C]/(\mu_2-\mu_1)_D$$

we have, since $\qquad (\mu_2)_F-(\mu_2)_C=1{\cdot}91B_2,$

and $\qquad (\mu_1)_F-(\mu_1)_C=1{\cdot}91B_1,$

therefore, $\qquad (\mu_2-\mu_1)_F-(\mu_2-\mu_1)_C=1{\cdot}91(B_2-B_1),$

or,

$$\frac{(\mu_2-\mu_1)_F-(\mu_2-\mu_1)_C}{\mu_2-\mu_1}=\frac{1{\cdot}91(B_2-B_1)}{\mu_2-\mu_1},$$

that is, $\qquad (B_2-B_1)/(\mu_2-\mu_1)=\beta/1{\cdot}91. \quad . \; . \; . \; . \; . \; . \; . \quad (16)$

Introducing the values given by equations (15) and (16) into equation (14), and writing g for the quantity $2\mu(\mu-1)/H(\sqrt{H}-1)$, we have

$$\frac{N_2}{N_1+N_2}=\frac{1+2g\bar{\omega}/1{\cdot}91\lambda^2}{1+2\beta/1{\cdot}91\lambda^2}$$

$$=\frac{0{\cdot}955\lambda^2+g\bar{\omega}}{0{\cdot}955\lambda^2+\beta}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (17)$$

or,

$$\frac{N_1}{N_1+N_2}=\frac{\beta-g\bar{\omega}}{0{\cdot}955\lambda^2+\beta} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (17a)$$

Here λ denotes the wavelength of the brightest light in the spectrum of the ½-watt gas-filled Osram lamp used as the source in these experiments. This has been investigated, for example, by Gibson and Tyndall, "Visibility of Radiant Energy" (Bureau of Standards, Sci. Papers, No. 145, v. 19, p. 131, 1923), who find the average luminosity curve for such a source reaches its maximum value at $\lambda=0{\cdot}58\ \mu$.

Again, according to Borgesius (Ann. d. Phys., v. 54, p. 221, 1895), β may be taken as 0·032 for all KCl solutions; 0·036 for NaCl solutions; and 0·042 for all solutions of KNO_3. (*See* also Hallwachs, Ann. d. Phys., v. 47, p. 380, 1892; and Dijken, Zeit. f. ph. Chem., v. 24, p. 81, 1897.)

Also it was found experimentally that for the compensating plates used, $\mu=1{\cdot}511$ and therefore $g=0{\cdot}488$.

Introducing these values into equations (17) and (17a), we obtain the results tabulated below :—

TABLE I.

	KCl.	NaCl.	KNO_3.
N_2/N_1	13·30	11·45	9·47
$N_1/(N_1+N_2)$	0·070	0·080	$0{\cdot}095_5$
$N_2/(N_1+N_2)$	0·930	0·920	$0{\cdot}904_5$

In the theoretical, ideal adjustment of the interferometer, which we have been considering, it has been assumed that the achromatic line, defined by the condition $d\varphi/d\lambda=0$, is restored from the displacement x'_1 to zero by a rotation of the compensator plate, and simultaneously the central fringe moves from x'_0 to x''_0—i.e., the recorded travel of the interferometer screw would correspond to a displacement of the central fringe equal to (N_1+N_2) fringe-widths of wavelength λ—the actual displacement of the central fringe by the solutions is N_2 fringe-widths. Hence, $N_2/(N_1+N_2)$ would be the factor by means of which the observations must be corrected for the " wandering " of the achromatic line if the theoretical adjustment of the instrument could be realised.

The expressions deduced in equations (17) and (17*a*), and evaluated in Table I, indicate that in the case of KCl solutions, for example, the achromatic line would move on with respect to the central fringe, one fringe-width for every 13·3 fringes in the displacement of the central fringe ; or, the displacement of the central fringe is 7 per cent. less than that of the achromatic line.

§ 4. PRACTICAL APPLICATION OF THE THEORY.

In the practical application of the theoretical expressions given above, however, the conditions mentioned will not, in general, be fulfilled ; for the achromatic line does not necessarily coincide in position with a fringe, and in compensating for the displacement of the fringes, we cannot restore the achromatic *line* to its undisplaced position, but only the nearest *fringe*, the achromatic fringe.

The modifications necessitated by this fact can be determined in the manner described below :—

N_1 is the number of fringe-widths in the final displacement of the central fringe, when the achromatic *line* is supposed to be restored to the zero position by a rotation of the compensator plate. This in general will not be an integral number.

But in experimental work, as the achromatic *fringe* is substituted for the achromatic line, it necessarily follows that there is always an integral number (say N'_1) of fringes in the final displacement of the central fringe when the compensator plate has been so rotated that the displacement of the achromatic *fringe* is zero.

Now it was found experimentally, by illuminating the slit S_0 (Figs. 1 and 2) by the appropriate source, instead of by white light, that a movement of the screw S (Fig. 2) through 0·00236 inch rotated the compensating plate C through such an angle that the green mercury fringes ($\lambda=0{\cdot}5461$ microns) were displaced through one fringe width. Similarly the movement of the screw was found to be 0·00255 inch for the fringes in Na light ($\lambda=0{\cdot}5893$ microns). In both cases the movement of the screw is almost exactly proportional to the wavelength. By interpolation we deduce the screw-movement necessary to displace the fringes in white light through one fringe space is 0·00250 inch. This method of screw calibration is much more accurate than attempting to estimate the movement in white light directly, since so much sharper fringes can be observed over so much greater a range of movement of the screw when monochromatic light is employed.

Therefore, if r is the final reading of the interferometer screw when the achromatic fringe has been restored to its undisplaced position, and r_0 is the zero reading—i.e., the reading of the screw when the same solution fills both parts of the cell, and the two compensating plates are exactly parallel, so that the achromatic fringes coincide in the upper and lower parts of the field of view of the eyepiece, then

$(r-r_0)/(0{\cdot}0025)$ = number of fringes in the total movement of the central fringe, during the process of " compensation "—i.e., $(r-r_0)/(0{\cdot}0025)=N'_1+N_2$ . . . . . . (18)

Now N_1 and N'_1 can be found as follows :—

As an approximation, take $N_1=N'_1$, and write

$$N_1 \doteqdot [N_1/(N_1+N_2)](N'_1+N_2) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \qquad (19)$$

The first factor is given by equation (17*a*), and its values have been included in Table I ; the second factor is obtained by equation (18), directly from the experimental observations, and thus the approximate value of N_1 is given by equation (19).

As the greatest displacement of the achromatic fringe from the achromatic line is half a fringe-width, it follows that this value of N_1 will also be correct to within this amount, and therefore N'_1 will be found as the nearest integer to N_1.

This expresses the fact that the shift of the achromatic fringe is not continuous but can only take place in steps of one fringe.

Now. from equation (18) we have

$$[(r-r_0)/0{\cdot}0025]-N'_1=N_2,$$

or, from equation (12), since in the apparatus used $E=1$ cm.

$$\{[(r-r_0)/0{\cdot}0025]-N'_1\} \times 0{\cdot}000058=\mu_2-\mu_1 \quad . \; . \; . \; . \; . \; . \; . \qquad (20)$$

where N'_1 is the nearest integer to

$$[(r-r_0)/0{\cdot}0025][(\beta-g\bar{\omega})/(0{\cdot}955\lambda^2+\beta)]$$

In order to demonstrate the magnitude of the quantities involved, some typical figures are given in Table II.

TABLE II.

Salt.	$r-r_0$	$N'_1+N_2=$ $(r-r_0)/0{\cdot}0025$	N_1 1st approx.	N'_1	N_2
NaCl	0·1496	59·84	4·80	5·00	54·84
	0·1245	49·80	4·00	4·00	45·80
	0·1495	59·80	4·80	5·00	54·80
	0·1350	54·00	4·34	4·00	50·00
	0·0993	39·72	3·19	3·00	36·72
	0·1035	41·40	3·32	3·00	38·40
K Cl	0·1177	47·08	3·29	3·00	44·08
	0·0963	38·52	2·69	3·00	35·52
	0·1154	46·16	3·23	3·00	43·16
	0·1038	41·52	2·90	3·00	38·52
	0·1542	61·68	4·31	4·00	57·68
	0·0798	31·92	2·23	2·00	29·92
KNO_3	0·0883	35·32	3·37	3·00	32·32
	0·1095	43·80	4·18	4·00	39·80
	*0·1440	57·60	5·50	5·50	52·10
	0·1316	52·64	5·02	5·00	47·64
	0·1382	55·28	5·27	5·00	50·28
	0·1113	44·52	4·25	4·00	40·52

* This reading is the mean of settings of two adjacent fringes of apparently equal " achromatism," on the zero of the eyepiece.

The author is indebted to Mr. J. Guild for this method of avoiding the error arising from want of coincidence of the achromatic line with the achromatic fringe.

From equation (20) we obtain the small difference in Refractive Index $(\mu_2-\mu_1)$ for two solutions of known small difference in concentration (n_2-n_1), for the brightest light in the source used—i.e., the value of $d\mu/dn$ for $\lambda=0{\cdot}58$ micron.

In the diffusion experiments to which these results are to be applied, it has been found useless to employ white light for the source, for the reason mentioned in Proc. Phys. Soc., v. 36, p. 322, 1924, and green mercury light ($\lambda=0{\cdot}5461$ micron) was used as the source. It is therefore necessary to make an estimate of the value of $d\mu/dn$ for $\lambda=0{\cdot}5461$ micron.

§ 5. CORRECTION FOR THE GREEN MERCURY LINE.

To correct the value of $(d\mu/dn)$ found by the interferometer for $\lambda=0{\cdot}58$ to its value for the green Hg line, $\lambda=0{\cdot}5461$, assume

$$\mu=A+B\lambda^{-2}$$

i.e. $$\left(\frac{d\mu}{dn}\right)_{\lambda_1}=\left(\frac{dA}{dn}\right)_{\lambda_1}+\lambda^{-2}{}_1\left(\frac{dB}{dn}\right)_{\lambda_1}\quad\text{for a wave-length } \lambda_1$$

and $$\left(\frac{d\mu}{dn}\right)_{\lambda_2}=\left(\frac{dA}{dn}\right)_{\lambda_2}+\lambda^{-2}{}_2\left(\frac{dB}{dn}\right)_{\lambda_2}\quad\text{for another wave-length } \lambda_2,$$

where A and B are functions of n, but not of λ.

From published values of μ and λ for solutions of sodium chloride of different concentrations it will be found that dA/dn is an approximate constant, having the value, independent of λ, of about 0·1541, and similarly dB/dn has an approximately constant value, independent of λ, equal to about $0{\cdot}2961\times10^{-10}$. Although it is not claimed that the accuracy of these figures is very high, yet the correction involved is so small that they are quite sufficient for our purpose. This is on account of the fact that the green mercury line is situated so near the brightest part of the spectrum.

Hence $$(d\mu/dn)_{\cdot58}=0{\cdot}1541+[1/(0{\cdot}000058)^2]\ 0{\cdot}2961\times10^{-10}$$
$$=0{\cdot}1628 \text{ approximately.}$$

Similarly $$(d\mu/dn)_{\cdot5461}=0{\cdot}1541+[1/(0{\cdot}00005461)^2]\ 0{\cdot}2961\times10^{-10}.$$

The difference, $(d\mu/dn)_{\cdot5461}-(d\mu/dn)_{\cdot58}$
$$=0{\cdot}2961\times10^{-10}[1/(0{\cdot}00005461)^2-1/(0{\cdot}000058)^2]$$
$$=0{\cdot}001127.$$

i.e., the value of $(d\mu/dn)$ for green mercury light is greater than that for wave length 0·58 micron by 0·00113 in 0·1628—that is, by 0·7 of one per cent.

Corrections of the same order of magnitude are found in the cases of KCl and KNO_3 solutions, so that the values of $(\mu_2-\mu_1)$ given by equation (20) must be increased by 0·7 per cent. to correct for the wavelength of the light employed.

§ 6. RESULTS FOR $(d\mu/dn)$.

It has been shown that if r_0 is the zero reading of the compensator screw, and r is its reading when the interferometer cell contains two solutions of slightly different concentrations n_1 and n_2, then the equation

$$\{[(r-r_0)/0{\cdot}0025]-N'_1\}\,0{\cdot}000058\times1{\cdot}007=d\mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (21)$$

expresses the small difference in their refractive index, where corrections have been introduced to amend the observations of the interferometer for (1) difference in dispersion of the media, and (2) the wavelength of the light employed.

An application of equation (21) enables the last column of Table III to be evaluated.

TABLE III.

Salt.	Concentrations.		Mean.	$r-r_0$	$d\mu/dn$
	n_1	n_2	n		
NaCl	0·2500	0·2273	0·239	0·1496	0·1409
	0·1667	0·1488	0·158	0·1245	0·1497
	0·1250	0·1042	0·115	0·1495	0·1537
	0·0625	0·0446	0·053	0·1350	0·1635
	0·0250	0·0125	0·019	0·0993	0·1716
	0·0125	0·0000	0·006	0·1035	0·1794
KCl............	0·2500	0·2273	0·239	0·1177	0·1132
	0·1667	0·1488	0·158	0·0963	0·1161
	0·1250	0·1042	0·115	0·1154	0·1210
	0·0625	0·0446	0·053	0·1038	0·1260
	0·0250	0·0000	0·0125	0·1542	0·1348
	0·0125	0·0000	0·006	0·0798	0·1397
KNO_3.........	0·2500	0·2273	0·239	0·0883	0·0830
	0·1667	0·1389	0·153	0·1095	0·0836
	0·1250	0·0893	0·107	0·1440	0·0852
	0·0625	0·0312	0·047	0·1316	0·0890
	0·0312	0·0000	0·0156	0·1382	0·0937
	0·0250	0·0000	0·0125	0·1113	0·0948

These values of $(d\mu/dn)$ are slightly different from those given in Proc. Phys. Soc., v. 36, p. 313, 1924, owing to a more correct value being adopted for the dominant wavelength of the light emitted by the Osram lamp.

All the experiments were conducted at temperatures between 18° and 19°C., and the solutions used were prepared by the use of standardised and calibrated pipettes of volumes 50, 25, 10 and 5 c.c.

The figures tabulated above in Table III are the mean results of a number of experiments which only differ from each other by quite small amounts, and they are plotted graphically in the curves of Fig. 6.

§ 7. DISCUSSION OF RESULTS.

In the application of these results to the author's experiments on diffusion, referred to in § 1, the following considerations should be borne in mind :—

(1) The difference $(d\mu)$ for two solutions of known difference in concentration

(*dn*) has been measured directly, instead of depending on the difference of two values of μ, each of which must be separately measured to a very high degree of accuracy.

(2) The interferometer measurements have been made in the same thermostat room, and at the same temperature, as the observations on diffusion to which they are to be applied.

(3) The correction for difference between the wavelength of the light used in the interferometer (λ=5800 Å) and that employed in the diffusion experiments (λ=5461 Å) is not large.

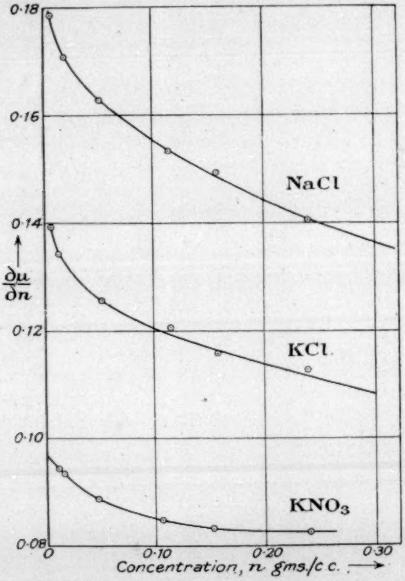

FIG. 6.

Accuracy.

The object with which this Paper is submitted is to describe a series of experiments constituting a subsidiary investigation in connexion with the optical method of studying diffusion recently described by the author before this Society, in which an estimate is required of the difference in refractive index of two solutions to the order of accuracy at present requisite in experimental work in diffusion—say, 2 per cent. or so.

The mean values of ($d\mu/dn$) given above in Table III are considered to be accurate to well within this limit, and if necessary this accuracy could be considerably increased, for almost all the error involved is due to the method which has usually been adopted in the preparation of the solutions—a method employed on account of its speed, its simplicity, and its reasonable accuracy—viz., the difference (*dn*) in concentration between the two solutions whose difference ($d\mu$) in refractive index is to be measured has usually been produced by the addition of a known volume of water to a known volume of the solution to be studied by means of calibrated pipettes. By the substitution of a chemical analysis of the solutions employed in the interferometer cell for the use of the pipettes, the error involved could be reduced to a considerable extent.

That a greater accuracy may now be thought to be not only desirable (in view of the interest connected with the diffusivity of dilute solutions), but also possible, indicates that a considerable advance has been made in measurements on diffusion ; but, as the author has previously stated, he feels that the diluter solutions should be the subject of further study, in order that the conclusions suggested by his recent work may be confirmed, and he intends to proceed with this investigation as soon as possible.

The work has been carried out in the Physical Laboratories of Birkbeck College, and the author is glad to express his indebtedness to Prof. A. Griffiths for his continued interest.

DISCUSSION.

Mr. J. GUILD: The author has called attention to a particular case of a general result in connexion with interferometer apparatus, namely, that complete compensation is only obtained in an interferometer if the materials and dimensions of the optical elements in each beam are identical. The Paper is of theoretical interest, but for practical purposes white light should not be used. It is possible, starting with an approximate knowledge of the quantity to be measured, to obtain values as accurate as may be required using monochromatic light. It is only necessary to limit the path difference introduced so that the approximate initial knowledge enables the number of fringes displaced to be calculated with an uncertainty of less than half a fringe. This gives the integral number of fringes in the displacement, the fraction being obtained from the actual setting. If desired, the more accurate value now obtained may be used as the starting point for another determination, using a correspondingly greater path difference. In this way a series of observations may be made each giving an accuracy of from five to ten times that of the preceding.

Mr. T. SMITH : The author's investigation shows how complicated such problems become when white light is used ; I should have thought monochromatic light preferable in this case. For a rigorous treatment it would be necessary to consider a further correction, arising out of the fact that the dominant wavelengths may be different in the two fringes which have to be compared, but possibly such a correction would be negligible for the purpose under consideration. For strict rigor it would also be necessary to consider dispersion in the eyepiece system.

AUTHOR's reply : I must express my gratitude to Mr. Guild for his suggestions as to the use of monochromatic light in the measurements under consideration. With regard to Mr. T. Smith's remarks, I have not allowed for a possible variation in the dominant wavelength in the two sets of fringes, but imagine this would produce only a very small modification in the results, of little importance in their application to diffusion.

XI.—A STUDY OF THE PRODUCTION OF "FLASHING" IN AIR ELECTRIC DISCHARGE TUBES.

By JAMES TAYLOR, *B.Sc.*, and WILLIAM CLARKSON, *B.Sc.*, Armstrong College, Newcastle-upon-Tyne.

Received May 9, 1924.

ABSTRACT.

The Paper deals with an extension of the study of "flashing" in discharge tubes* containing air instead of the usual mixture of neon and helium.

The volt-ampère characteristics for the air discharge tubes are shown to be of the general form,

$$i = k(V - M),$$

where i is the current through the tube, k the conductance of the tube, V the voltage across the electrodes, and M a constant, theoretically of the value of the kathode fall of potential V_A.

The relation for the time of "flashing" T is given theoretically by

$$T = CR\,.\log_e \frac{E - V_B}{E - V_C} + C\Delta\,.\log_e \frac{V_C - V_A}{V_B - V_A}$$

where C is the capacity in microfarads across the tube, R is the resistance in megohms, in series with the tube, E, V_C, V_B, the charging, upper critical, and lower critical voltages respectively, and Δ a constant.

This relation is generally confirmed by experiment.

It is shown that, as in the case of the "osglim" lamp, there is a critical resistance R_c below which no "flashing" is possible, given by

$$R_c = \frac{E - V_B}{k(V_B - V_A)}.$$

INTRODUCTION.

THE following work is a continuation of the study of "flashing" of the neon tube,† which contains a mixture of the rare monatomic gases neon and helium; it was of considerable interest, therefore, to investigate the phenomenon in a diatomic molecular mixture like air, and to see if the same laws were valid.

It is to be observed that the theory put forward previously is theoretically applicable solely to discharge tubes in which the positive column is absent (so that no repetition of the dark space phenomena occur); thus the electrodes of the tube must not be more than some few mm.s apart.

Experiment shows that in the case of air discharge tubes of very varied types the phenomenon of "flashes" may be readily obtained. The range of pressures for which "flashes" are possible is very wide (0·35 to 40 mm. in these experiments) for a voltage range of some 400 up to 1,000 volts. Furthermore, "flashes" of almost any period can be obtained.

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* First observed by Pearson and Anson, in the case of the neon-filled "osglim" lamp (Proc. Phys. Soc., Vol. 34, p. 204.)

† Pearson and Anson, loc. cit. Taylor and Clarkson, Journ. Scien. Instrs., Vol. 1, p. 173; also Proc. Phys. Soc., Vol. 36, Pt. 4, p. 269.

EXAMINATION OF THE UPPER AND LOWER CRITICAL VOLTAGES.

It was found that for the various discharge tubes, under different given conditions of temperature and pressure, a definite potential across the electrodes—the "sparking" potential or upper critical voltage V_C—was required to initiate discharge. V_C was definite and repeatable to within one or two volts, provided the discharge tube remained under identical conditions.

It is important that V_C should be measured under conditions similar to those which will exist during experiments (for method, see later).

Graph (1) of Fig. (1) shows the variation of V_C with pressure; the curve is of similar form to that obtained for the "sparking" potential of gases. There is a flat minimum in the neighbourhood of 2·5 mm., and at greater pressures than 3·5 mm.s V_C becomes a linear function of the pressure.

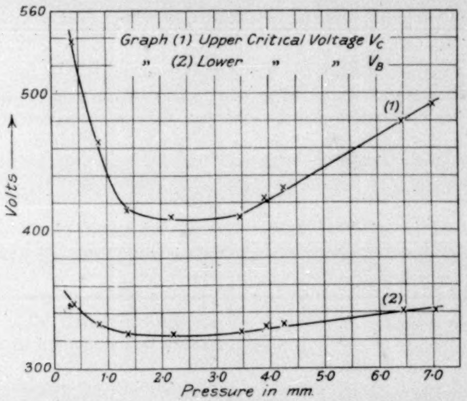

FIG. 1.—GRAPHS SHOWING THE VARIATION OF THE UPPER AND LOWER CRITICAL VOLTAGES (FOR AIR), WITH PRESSURE.

For air, as for other gases, there is a lower critical voltage V_B below which no discharge may be maintained. V_B is of constant and repeatable value for a definite pressure, under given conditions. Graph (2) of Fig. (1) shows the variation of V_B with pressure. There is a flat minimum at the same pressure as the minimum for V_C, and at higher pressures V_B increases linearly, with a small slope compared with that for V_C.

EXPERIMENTAL METHOD FOR THE DETERMINATION OF V_C AND V_B.

Fig. 2 gives the experimental arrangement used for the determination of V_B and V_C. With suitable values of the capacity C and the resistance R, "flashing" occurs. E, the charging voltage, is gradually lowered in value (R being reduced at the same time to decrease the time period of "flashes") till no further "flashing" occurs; the value of E is then that of the upper critical voltage. It is important that V_C should be measured under conditions similar to those which will exist during experiments.

E is then raised so as to be greater than V_C, so that "flashing" occurs, R is reduced to the critical value R_c (*see* theoretical discussion), and the discharge becomes continuous. The voltage across the tube at this adjustment is V_B[*] (an electrostatic voltmeter is employed for the measurement of the voltage).

The Volt-Ampère Characteristics of Air Discharge Tubes.

One of the present writers obtained theoretically, on certain assumptions, a current-voltage law of the following form[†] (no resistance being in series with the discharge tube) :—

$$i=k(V-V_A) \qquad \qquad (1)$$

where i is the current through the tube, V the voltage across the electrodes, V_A the kathode fall of potential (approximately), and k is a constant depending upon the area of the kathode utilised during the discharge, and may be termed the conductance of the tube.

The above relation agrees substantially with that obtained for the neon lamp,[‡] and it should be applicable to any discharge tube in which the positive column is absent.

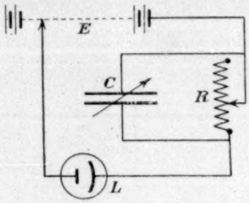

FIG. 2.

In the present experiments the characteristics for the air discharge tubes were determined, between the upper and lower critical voltages. Although the method was by no means free from objections, the volt-ampère characteristics (Fig. 3) for different pressures were approximately of the linear form,

$$i=k(V-M) \qquad \qquad (2)$$

M, the intercept on the voltage axis, which should be of the value V_A, is almost constant for different pressures and is of the order of magnitude expected.

In the case of many tubes the results were, however, very unsatisfactory. Provided, however, that M is not interpreted literally as V_A, the kathode fall of potential, but rather as a constant of meaning at present undetermined, the results of observation agree substantially with the theoretical relationship of equation (1).

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[*] Taylor and Clarkson, Proc. Phys. Soc., Vol. 36, Part 4, pp. 273, 276.

[†] Journ. Scient. Instrs., T. and C., loc. cit.

[‡] P. and A., loc. cit.

EXPERIMENTAL METHOD FOR THE DETERMINATION OF THE VOLT-AMPERE CHARACTERISTICS.

Fig. 4 indicates the experimental arrangement for the determination of these characteristics. A potentiometer was used so that the voltage across the tube could be varied. *A* was a milliammeter for measurement of the current through the tube, E_v, an electrostatic voltmeter for determining the potential across the tube terminals, and *R* was a variable resistance.

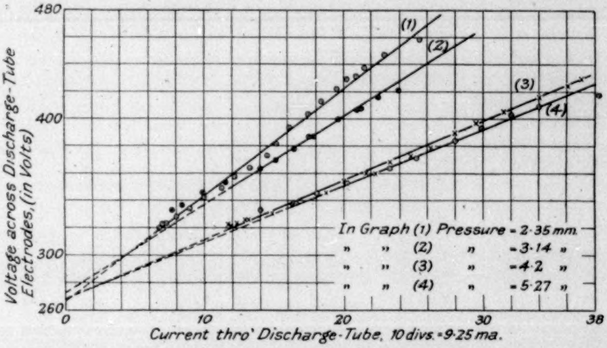

FIG. 3.—GRAPHS OF THE VOLT-AMPERE CHARACTERISTICS FOR AIR AT DIFFERENT PRESSURES.

A preliminary experiment was made (with *R* adjusted to be several thousand ohms) to ascertain the approximate values of V_B and V_C.

With *R* of zero value, the voltage *E* was adjusted to be just less than V_C, and was then stepped up gradually until the discharge started. As soon as the current through the discharge tube had fallen to a steady value, readings of the voltage and current were obtained simultaneously. The voltage was then progressively diminished, whilst the values of the current were obtained, until it had diminished to the lower critical voltage, when the current dropped abruptly to zero.

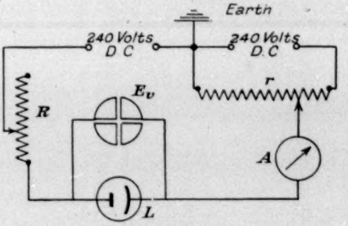

FIG. 4.

It is well known that the value of the current through a discharge tube under these conditions of running on a steady voltage, falls off almost exponentially from its initial value, with time, to a definite assymptotic value.[*] This would appear to vitiate the value of the above results, for it is evidently the instantaneous value of the current, during the short luminous period, that must be taken into account in the " flashing " phenomena.

It was found, however, provided the current was not more than some 10 or 15 milliamperes, that the initial values of the current were not greatly in excess of the final values, and that the results were repeatable to within a few per cent. When the discharge became sufficiently intense to produce great local heating, erratic results were obtained.

On the whole, it is reasonable to assume that the magnitudes only, and not the form of the volt-ampère characteristics, will differ in the two cases of the final and the instantaneous values.

THE THEORY OF " FLASHING."

There are two positions for the capacity for which " flashing " may be obtained. The first position is where the capacity is across the tube terminals, and the second position is where the capacity is across the resistance (Fig. 2).

The theory of the first position has been put forward in previous Papers,[†] the time period being given by the relation

$$T=CR\,.\,\log_e\frac{E-V_{\mathrm{B}}}{E-V_{\mathrm{C}}}+\frac{CR}{1+kR}\,.\,\log_e\frac{V_{\mathrm{C}}-\dfrac{E+kRV_{\mathrm{A}}}{I+k\mathrm{R}}}{V_{\mathrm{B}}-\dfrac{E+kRV_{\mathrm{A}}}{1+kR}}\quad.\;.\;.\;.\;.\;.\quad(3)$$

where C is the capacity across the tube in microfarads, R is the series resistance in megohms, and E is the charging voltage. (Other terms have the same meaning as before.)

The second position of the condenser is characterised by a time period relation of exactly the same form as that given by equation (3).

The condensers available made it more convenient, in the present experiments, to use the second position of the capacity. When (as is usually the case) kR is large compared with unity, the relation of equation (3) may be approximated to the more simple form[‡]

$$T=CR\,.\,\log_e\frac{E-V_{\mathrm{B}}}{E-V_{\mathrm{C}}}+\frac{C}{k}\,.\,\log_e\frac{V_{\mathrm{C}}-V_{\mathrm{A}}}{V_{\mathrm{B}}-V_{\mathrm{A}}}\quad.\;.\;.\;.\;.\;.\;.\;.\;.\;.\quad(4)$$

The time period should, therefore, be directly proportional to C, over a wide range of capacities ; further, since the first term of equation (3) is very large compared with the second, T should be a linear function of R, over a wide range of resistances. When, however, R becomes small, the product kR progressively diminishes with R, and the second term of equation (3) increases rapidly, so that the linear relation is departed from, T being of greater value than that given by a linear form.

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[*] P. and A., loc. cit. Shaxby and Evans, Proc. Phys. Soc., Vol. 36, Pt. 4, p. 253.

[†] P. and A., loc. cit. T. and C., Journ. Scient. Instrs., loc. cit.

[‡] T. and C., Journ. Scient. Instrs., loc. cit.

It is further evident if CR is fixed, that T should be a linear function of the logarithm term

$$\log_e \frac{E-V_B}{E-V_C}$$

THE CRITICAL RESISTANCE R_c FOR "FLASHING."

It was shown previously that there is a limiting value for R, called the critical resistance R_c, below which no "flashing" is possible, given by the relation,*

$$R_c=\frac{E-V_B}{k(V_B-V_A)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The second capacity position is characterised by a critical resistance, given by an exactly similar relation. R_c should therefore be a linear function of E, intercepting the axis of voltage, at a value V_B.

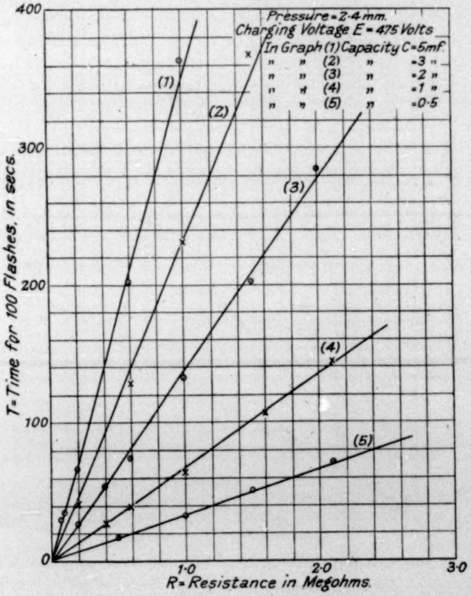

FIG. 5.—GRAPHS SHOWING RELATIONSHIP BETWEEN THE TIME OF FLASHING T AND THE RESISTANCE R FOR DIFFERENT CAPACITIES.

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* T. and C., Proc. Phys. Soc., loc. cit.

EXPERIMENTAL EXAMINATION OF THE RELATIONS OF THE TIME PERIOD OF "FLASH" TO RESISTANCE AND CAPACITY.

The method of experiment was exactly similar to that adopted in the experiments of previous Papers, except that all the tubes used were attached to a Toepler pump in order that the pressure of the air in them could be varied. The pressure was measured by means of a McLeod gauge.

In order to ensure dryness for the air used, phosphorus pentoxide drying-tubes were attached to the discharge tube apparatus.

The graphs of Figs. 5 and 6 show the relationship between the period of "flashing" T, and the resistance R for different capacities, and at two different pressures. (The examples are taken from a numerous series, at different voltages, but they were all of exactly the same form so that inclusion of the whole series is unnecessary.) The discharge tube was that used in the volt-ampere characteristic, and the critical voltage determinations, and consisted of an ordinary "I" pattern

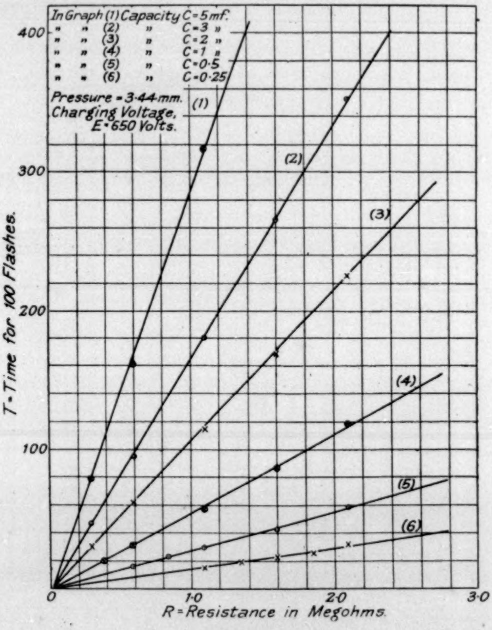

FIG. 6.—GRAPHS SHOWING RELATIONSHIP BETWEEN THE TIME OF FLASHING T AND THE RESISTANCE R FOR DIFFERENT CAPACITIES.

" osglim " lamp which was sealed on to the apparatus for exhausting of air. The dimensions were approximately :—

Kathode plate (iron) 3·5 by 0·5 cm. Anode cylinder 1·5 by 0·2 cm. The cylinder was parallel to the kathode (along same axis) and the minimum distance between them was 0·15 cm.

The graphs are all linear functions, except when R is small and near the critical resistance value ; this relation holds over a wide range of pressures. Further, the intercepts on the axis of resistance are small, in accordance with the relation of equation (4).

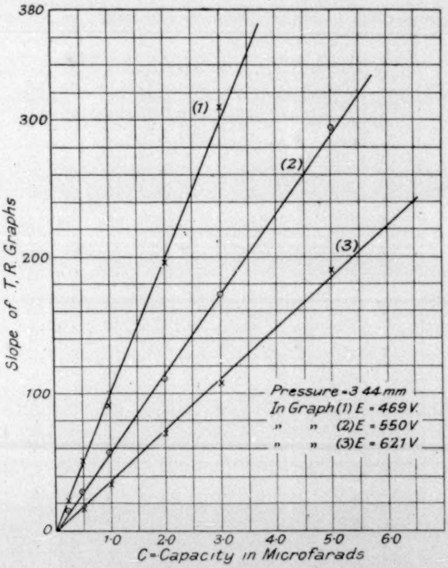

FIG. 7.—GRAPHS SHOWING RELATIONSHIP BETWEEN THE SLOPES OF THE T, R GRAPHS AND CAPACITIES FOR DIFFERENT VOLTAGES E.

The graphs of Fig. 7 are examples of the type of relationship between the slope of the T, R graphs and the capacity C. They are linear, and intercept the axis of capacity at points very near the origin, in accordance with the theory.

The graphs of Fig. 8, taken for a discharge tube with copper disc electrodes (distance apart of electrodes 0·5 mm., diameter of electrodes 3·0 cm.) show the curving off of the T, R graphs as R decreases, for various pressures.

In the case, however, of the variation of T with the logarithm factor, an anomaly arises. T was found approximately to vary linearly with this factor (*see* graphs of Fig. 9), but the relation was of the form

$$T=A\left(\log_e \frac{E-V_B}{E-V_C}-B\right) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (5)$$

where A and B are constants.

B is of small magnitude, but A, the slope of the T, log. factor graphs, was always found to be greatly in excess of the theoretical value CR. (In the graphs of Fig. 9, since the ordinates are the time periods $100T$, for C and R both unity, this time period should be very approximately equal to $100 \, . \log_e \frac{E-V_B}{E-V_C}$).

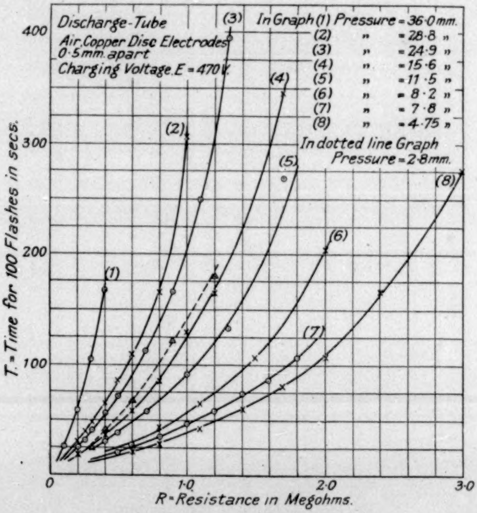

FIG. 8.—GRAPHS SHOWING RELATIONSHIP BETWEEN T, THE TIME OF "FLASHING," AND THE RESISTANCE R FOR DIFFERENT PRESSURES.

The discrepancy remains very great even though the duration of the luminous period of "flashing" is taken into consideration. In a few cases the time period as calculated from equation (4) agreed very closely with the values obtained experimentally; these however, were the exceptions.

The anomaly may be bound up in any variable factors of equation (4), or in a combination of them, and it has not been found possible, up to the present, to elucidate it.

Experiments with various types of air discharge tubes showed definitely that

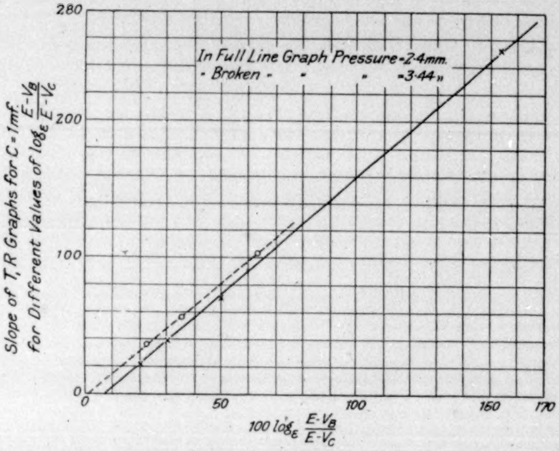

FIG. 9.—GRAPHS SHOWING THE RELATIONSHIP BETWEEN THE SLOPE OF THE T, R GRAPHS FOR $C=1$ MICROFARAD (FOR DIFFERENT VALUES OF E) AND $\log_e \dfrac{E-V_B}{E-V_C}$.

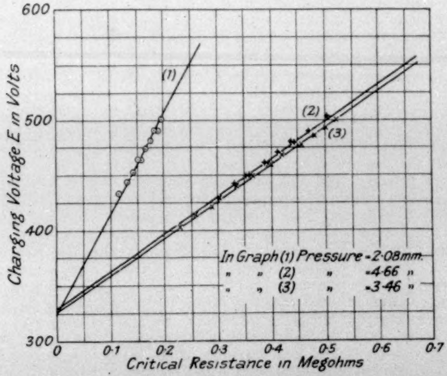

FIG. 10.—GRAPHS SHOWING RELATIONSHIP BETWEEN R_c, THE CRITICAL RESISTANCE, AND E THE CHARGING VOLTAGE.

there was always a critical resistance for "flashing." In many cases the magnitude of this critical resistance was not more than a few thousand ohms.

It was also ascertained that at the critical resistance, the voltage across the tube fell to the lower critical value V_B, and that R_c was a linear function of the charging voltage E.

The graphs of Fig. 10 show the form of the R_c, E graphs, for the capacity across the resistance, in the case of a wireless valve improvised as a discharge tube. They are straight lines, intercepting the voltage axis approximately in accordance with the relation of equation (5). The results obtained with the majority of the tubes were neither very definite nor conclusive, and it is proposed to investigate the problem more fully.

The authors beg to acknowledge their indebtedness to Prof. G. W. Todd of Armstrong College, under whose supervision the experiments were carried out.

DISCUSSION.

Dr. D. Owen: While the authors have made interesting observations on the existence of the phenomenon of flashing in air discharge tubes, and have pointed out the existence of a critical resistance, it is necessary to remark that their observations were made under limited conditions. For instance, the rate of flashing seems not to have exceeded about 5 per second, and the capacities used were in every case large. The assumption also of a straight volt-ampere characteristic is only very partially true.

Mr. E. J. Evans: The phenomenon of flashing in discharge tubes was shown by me before the Society in February, 1923. I would like to ask the authors what was their source of potential in the experiments, and how they measured the frequency of the flashes. In Figs. 6 and 8 the authors show that an alteration of electrodes produced an anomalous result. I have observed in my own work using discharge tubes of different lengths and shapes that the results obtained are not in agreement with those given in this Paper, e.g., I obtain intermittence without having any capacity in parallel with either the tube or resistance, while the resistance in series is decidedly lower than the critical values given. I do not get the linear relationship for the volt-ampère characteristic. I find also that at high gas pressure the discharge is intermittent and that the rate of intermittence increases with a decrease of pressure. At a certain critical pressure the luminous period increases and on further reducing the pressure the discharge becomes apparently continuous. Still further reduction of pressure produces intermittence again, the frequency now being low.

Prof. E. V. Appleton and **Mr. Emeléus** (communicated): The results described in the above Paper are in agreement with those obtained by Appleton, Emeléus and Barnett (Proc. Camb. Phil. Soc., XXII, p. 434, 1924), when studying the action of Rutherford and Geiger's apparatus for counting α and β-particles electrically. It was found that the transient discharge between a point and a plane, initiated by an ionizing particle in air at reduced pressure, was one unit of the sustained periodic discharge which set in at sufficiently high voltages or low pressures, and that the oscillations were very similar to the "flashing" of a neon lamp. Linear relations between capacity and time period and the characteristic variation of period with resistance were described together with oscillographic determinations of the wave-forms. The counter may be set up with the condenser in either of the two positions. The interesting result that the lower critical voltage decreases when the capacity is sufficiently reduced is in concord with our ballistic measurements. For capacities below a certain amount the quantity of electricity circulating for each discharge actually increased as the capacity decreased. This was attributed at the time to a change in the conductance K, but it is now clear that it is the change in the lower critical voltage which brings about this result. That the latter view is the correct one is verified by direct measurements of the fall of potential across the counter for single discharges made by Geiger (Zeit. für Phys., 27, 7, 1924). Our results have also shown that the effective capacity of the discharge tube and condenser is larger than that calculated from the geometry of the system, and that "flashing" can take place when the positive column is present.

AUTHORS' reply (communicated): The work was necessarily restricted owing to lack of suitable sources of voltage and capacities. The linearity or non-linearity of the volt-ampère characteristics does not gravely affect the general phenomena of "flashing," except at points in the vicinity of the critical resistance. This is seen to be true from a consideration of equations (3) and (4), the first term, as a rule, being of large magnitude compared with the second, and also independent of the form of the characteristic.

In the Paper the portion on the critical resistance is merely illustrative, and as indicated in the addendum a more exact study is given elsewhere (Taylor and Clarkson, Phil. Mag., Vol. 49, No. 290, p. 336). It is shown in the Paper referred to that the linearity of the E, R_c graphs is independent of the form of the volt-ampère characteristics.

In reply to Mr. E. J. Evans, we regret that we are not in a position to discuss the results put forward, since there is no indication of the types of discharge tubes used, the gas employed or the working pressures, nor is it stated what methods were adopted for the observing of successful experimental conditions. We may be permitted, however, to point out that we ourselves have experimented on numerous tubes which gave entirely anomalous and erratic results. Sources of trouble are extremely numerous. Some of the inherent experimental difficulties are described in the Paper referred to (T. and C., Phil. Mag., loc. cit., pp. 338, 345, 353). In addition, the tubes used for the "flashing" relations must have kathodes of suitable size (if linear relations are to be obtained), so that the working conditions are not situate in the range where great change of the critical constants with diminution of capacity occurs. It is obviously impossible to apply the relation of equation (2) to the "flashing" of a tube, if the critical constants are changing rapidly with capacity. (For change of constants under different conditions, see T. and C., Phil. Mag., loc. cit.; Taylor and Stephenson, Journ. Sci. Instrs., Vol. 11, No. 2, p. 50; and Taylor, Clarkson and Stephenson, Journ. Sci. Instrs., Vol. II, No. 5, p. 154.)

Grave departures from linear relations are frequently due to a "lag" in the discharge behind the voltage tending to produce it, owing to insufficiency of ionization within the tube. This may be overcome by the utilisation of an external ionizing agent, such as γ-rays (*see* T. and S., loc. cit.; T., C. and S., loc. cit.). We do not know whether Mr. Evans utilised tubes in which there were positive columns. If this were the case, it is obviously impossible to compare the results with ours, since we confined our attention definitely to tubes fulfilling the conditions laid down in the Paper.

We do not agree that Figs. 6 and 7 show anomalous results. The discharge tube used in obtaining the results of Fig. 7 had a conductance greater than those of the other tubes we employed, and the working pressures were much greater. It is apparent, therefore, that the second term of equation (3) cannot be taken as a constant, especially when it is remembered that R_c is greater for higher pressures, and as this value is approached the second term (which is the luminous period time) becomes greater and greater. Further, we had ascertained by actual experiment that for this particular tube, at a constant pressure and fixed values of R and C, the time period was a linear function of the logarithm term, in accordance with equation (3).

The observation of the production of "flashing" or intermittance in tubes without any capacity in parallel is interesting (the magnitude of the critical resistance has no significance in comparing results, however, since it is a function of the conductance of the tube, and has an indefinite range of possible values). It has been observed previously, however, by Ives, in work on air discharge tubes on the so-called magnetic rays of Righi, by Aston and Watson in the case of rare gas discharge tubes, and by others. In most of the cases where slow "flashing" under these conditions has been observed there has been a positive column, and the conditions of the two cases are scarcely comparable. In the case of high-frequency flashes, if these are the type referred to, the residual capacity of the circuit and discharge tube electrodes is usually sufficiently great to account for them. When the value of the charging voltage E is very near to the upper critical voltage value, it is seen, from a consideration of equation (3), that the phenomena of "flashing" could occur at slow frequencies even with low capacity and resistance values. We suggest that the explanation of Mr. Evans's last results may be deduced from a consideration of equation (3), together with the empirically obtained curves of the variation of the upper and lower critical voltages with the pressure (Fig. 1), and the critical resistance condition for the maintenance of "flashing" in the circuit.

The authors are greatly indebted to Prof. E. V. Appleton and Mr. Emeléus for their communication, and it is hoped to be able to carry out some of the experiments described in their Paper on the particular type of discharge tube that we have used.

XII.—NOTES CONCERNING THE SPRENGEL PUMP.

By J. J. Manley, *M.A.*, Research Fellow, Magdalen College, Oxford.

Abstract.

The Paper describes an improved design of the modified Sprengel pump described by the author in the Proceedings of the Physical Society, Vol. 36, p. 291. The walls of the pump are freed from gas skins by electric means, and the shattering of the mercury pellets (which tends to liberate gas) is checked by a special construction of the fall tube.

In a former Paper[*] I showed how by an electrical method the interior surfaces of a Sprengel pump and also the inflowing mercury could be freed, the one from the gas-grown skins and the other from adsorbed and condensed gases. It was also shown that in the absence of gases obtainable from the glass walls of the pump and possibly from the mercury, the pump acquired a much greater and in all probability the maximum efficiency attainable.

Recently, other pumps have been constructed ; each of these was given a form differing but slightly from that of the parent pump of my earlier Paper. After experimenting with these, choice was made of that illustrated in Fig. 1. The chambers *A* and *B* of this pump are 20 cm. long and 2 cm. wide ; their upper portions are closely fitted with external electrodes *C*, *D* of tinfoil, 10 cm. long. These electrodes are formed and attached as described in my former communication ; but on account of its greater reliability, Canada balsam, rendered conveniently liquid by adding xylene, was substituted for the alcoholic solution of shellac used in previous cases. Internal electrodes of mercury are provided by fusing platinum wires *w*, *w*, into the lower part of each chamber ; placed thus, the ends of the wires are completely submerged when the pump is fully charged with mercury. The tubes *Q* and *R* are fused, the one to a barometric trap and the other to a Töpler pump ; the trap is placed between the head of the Sprengel pump, and any vessel we may wish to evacuate. This new pump is prepared for use by proceeding in the way already described.[*]

When the coil *J* is activated by a battery *E* of three accumulators in series, the ensuing glow discharge within the chambers is chiefly concentrated in the spaces between the mercury and the interiors of the tinfoil electrodes ; this discharge erupts the gas-grown skins of the interior surfaces, and possibly some of the gases adsorbed by the mercury. By concurrently using the Töpler pump and coil, the chambers are quickly evacuated. The Sprengel pump is ready for action when a green fluorescent glow appears in the chambers *A*, *B*. From this time onwards the coil and the auxiliary pump are seldom required for more than a few seconds daily. It therefore appears that the mercury used for working the pump carries little or no air with it ; or, if it does so, such air is not readily detached by the glow discharge in vacuo ; hence we conclude that the primary source of the air which under usual conditions, limits the efficiency of a Sprengel pump, must be the internal surfaces of the pump itself.

The bulb *P* contains phosphorus pentoxide purified by ozone.[†] The oxide is so placed that the chambers *A*, *B* and the mercury within them, are kept dry; the oxide also removes moisture from the Töpler pump, and the connecting tube *R*. The new pump is to be preferred to the original in that it is simpler in form, equally efficient, and also silent in action. In the older pump, the mercury falls from a glass jet at the top of either chamber, on to the surface of the metal beneath, and so produces considerable noise. I conclude by describing two minor improvements.

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[*] Proc. Phys. Soc., Vol. 36, Part 4, p. 291.

[‡] Trans. Chem. Soc., Vol. 121, p. 331.

(*α*) With some pumps there is at times a tendency for the mercury to stick and accumulate in the fall-tube and head; and frequently a difficulty is experienced in re-starting its flow. This defect is remedied by making the head-piece about 1 cm. wide, as shown at *S*.

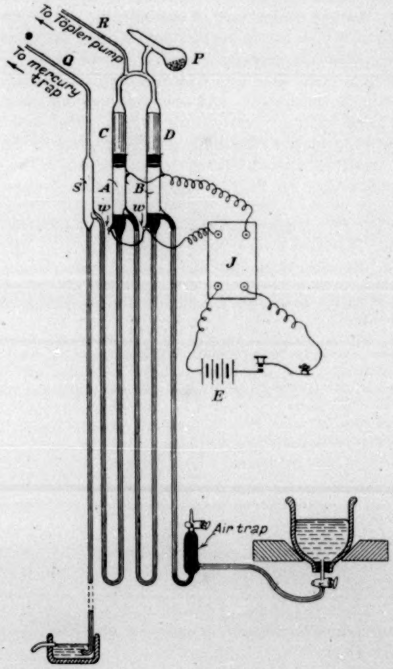


FIG. 1.

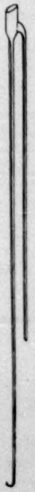


FIG. 2.

(*β*) When the vacuum is high, the pellets of mercury as they drop in the fall tube, are often shattered, and as a result the minute gas bubbles which are being removed, are enabled to re-enter the main apparatus. This somewhat serious imperfection can be almost entirely obviated by constructing the fall tube in two sections. The lower or main section is as usual cylindrical; its length is 72 cm. and its bore 0·8 mm. or less. The upper section must be made by the worker. It is formed by first thickening a suitable portion of a wide glass tube, and then drawing it out so that it is given a conical form. At the widest end, the bore should be about 3 mm., and at the narrowest 1 mm. The two tubes when fused together form a complete fall-tube as shown in Fig. 2.

It will be seen that the success of this device depends upon the fact that the pellets of mercury as they enter the conical part of the fall-tube, trap off in succession a maximum volume of gas; as the gas is pushed downwards, its column narrows continuously until the gas enters the cylindrical part of the tube; at the same time the pellets of mercury become fairly long and narrow cylinders, and these are not easily shattered. Ordinarily the minute quantity of gas trapped off towards the end, instead of forming a complete cylinder between the falling pellet and the column beneath, is seen to be present as an imperfect little bubble clinging to the wall of the fall-tube; if the bubble remains, it is ultimately joined by others which coalesce with it, and produce a complete disc-like bubble which then journeys on and is finally expelled.

DISCUSSION.

Prof. C. V. BOYS said that the troubles which the author had dealt with had also occupied the attention of Rood some fifty years ago. The latter had sought to overcome them by arranging for the mercury first to fall in drops in a vacuum chamber to rid it of gas, and subsequently, at the end of the effective part of its fall, to impinge on the side of a tube so curved as to check the fall without shattering the drops.

AUTHOR's reply: Rood's device as described by Prof. C. V. Boys was ingenious, and would, according to my own experience, be distinctly beneficial. When drops of mercury fall within a vacuum, adherent gases are detached, and this alone leads to a decided gain in efficiency. But for my own particular work, the success of which demanded the absence of adsorbed gases and gas-grown skins, a more drastic scheme was imperative. The plans described in this present Paper, and also in earlier ones upon the same subject, appear to ensure a sufficiently complete removal of all matter capable of assuming the gaseous state.

XIII.—THE THOMSON EFFECT IN COPPER, IRON, AND CARBON STEELS.

By J. Young, *B.Sc., A.R.C.Sc., F.R.A.S.*, Lecturer in Physics in the University of Birmingham.

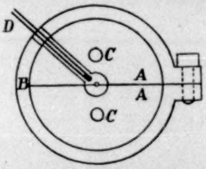
Received January **11, 1925.**

Abstract.

The apparatus described by Nettleton in the Proceedings of the Physical Society, April 1922, has been employed for the study of the Thomson effect in copper, iron, and carbon steels. It was found that different results were obtained according as the electric current in the wire under test flowed with or against the temperature gradient. The results are compared with those obtained by other workers for the thermo-electric power of the materials investigated.

In view of certain magnetic and electrical phenomena which are exhibited by carbon steels at or in the neighbourhood of definite temperatures, for example, the change in magnetic permeability of such a steel at 215°C., it was suggested to me by Prof. S. W. J. Smith that similar changes very probably exist in other properties of the material and that it might be worth trying if any such variation was detectable in the Thomson effect. At that time (November, 1922) Nettleton had succeeded in perfecting his apparatus for the measurement of this effect in wires,* and it was considered opportune to use his method in this investigation.

Accordingly, an apparatus was constructed, consisting of two electric heaters, *A* and *B*, to the copper cores of which two copper discs, A^1 and B^1, could be tightly fixed by screws. Each disc was 4·8 cm. in diameter and 0·55 cm. in thickness. The experimental wire was silver-soldered to these discs, in the manner described by Nettleton, care being taken to see that the faces of A^1 and B^1 into which the wire penetrated were free from solder on their surfaces near the junctions. In this way the effect of Peltier heat creeping along the wire and so upsetting the temperature gradient in it was made as small as possible.



AA. Copper disc split across diameter.
B. Steel band for clamping.
CC. Screw holes for fixing disc to heater.
D. Thermojunction.

Fig. 1.

In some of the later experiments a method of fixing the wire by clamping was tried. The discs were split (*see* Fig. 1), the two halves being held together by a steel ring which could be tightened up by means of a screw. This method of holding the experimental wire was found most satisfactory, and it gave substantially the same results as were obtained in the experiments in which the wire was fixed by soldering.

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* Proc. Phys. Soc., April (1922).

The general method of experimenting has already been very fully discussed by Nettleton in his papers, but for completeness the following short description is given.

A current, i_1, is passed through the experimental wire, the ends, A and B, being at steady temperatures, θ_1 and θ_2, respectively. When a steady state is reached, as indicated by a small bolometer coil wound centrally on the wire, the current is reversed and at the same time slightly reduced or increased to a value, i_2, such that, after an interval of about one minute, during which the apparatus is allowed to settle down, no change is detected in the temperature of the wire. This condition of balance is found by trial, the change, i_2-i_1, being adjusted by drawing plugs from a resistance box. The scheme of electrical connexions required to do this automatically by reversing the current is shown in Fig. 2. The principle underlying the method is thus to compensate the gain in Thomson heat on reversal by the loss of Joulean heat on reduction of the current from i_2 to i_1.

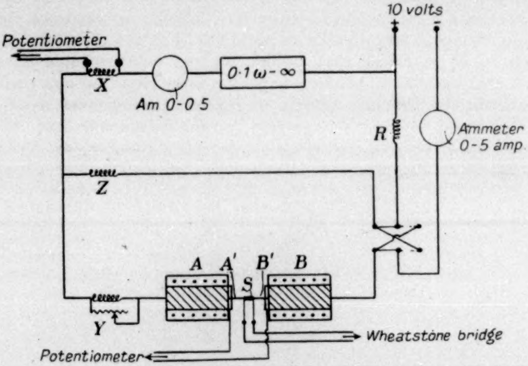

FIG. 2.—DIAGRAM OF APPARATUS.

The complete theory of the Thomson effect in an electrically heated wire was given by Nettleton in 1916.* He found that if the emissivity of the wire was sufficiently small,

$$\sigma_\theta = R(i_2-i_1)/JU \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

σ_θ=coeff. of the Thomson effect at the temperature $\dfrac{\theta_1+\theta_2}{2}$.

R=actual resistance of the wire.

$U=\theta_2-\theta_1$.

J=mechanical equivalent of 1 calorie.

It is not proposed to discuss here the corrections which must be made when the emissivity ceases to be negligible.

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* Proc. Phys. Soc., 29, p. 68.

Under good working conditions, the emissivity must be small and nearly constant as the lagging between the two heaters will take up roughly the same temperature gradient as the wire itself. Nettleton apparently satisfied himself that under these conditions the use of the simple formula was justified, and it has been used entirely in the calculation of the results given in this Paper. In any case, some experiments were made in which the experimental wire had a different diameter, a factor which affects the emissivity term, and the results justified the use of the simple formula.

METHOD OF MAKING OBSERVATIONS.

The two heaters were first adjusted to approximately steady temperatures, differing by about 50°C., as indicated on two mercury thermometers inserted into holes in the respective heaters. The main current of 2·7 amperes obtained from a 10 volt battery was then started, and after a minute or two the bolometer coil was put into action. This coil had a resistance of about 2 ohms at ordinary temperatures and consisted of three layers of No. 40 silk-covered copper wire. Its length was usually 1 centimetre, and it was further insulated by being painted with Ohmaline varnish, a plastic varnish which was found most satisfactory even at a temperature of 300°C.

The Wheatstone bridge of which the bolometer coil formed the fourth arm was then balanced and the heating currents adjusted until the balance remained permanent. The galvanometer key was kept closed continuously during the latter observation. At first a slow creep of the galvanometer spot, due to the continual heating up of the asbestos packing protecting the experimental wire, gave great trouble. This, however, was got rid of to a great extent by shortening the experimental wire to about 4·5 cm., which, while lessening the sensitivity of the measurement, gives the more valuable advantage of steadiness and freedom from temperature storms. In some of the later experiments a compensating device was used to overcome this difficulty, a small coil similar to the bolometer and of equal resistance being placed in the asbestos packing and made an adjacent arm in the Wheatstone network. This arrangement gave great steadiness and was very convenient to work with. The current used in the bolometer was about 0·05 amp. and was supplied from a 2 volt accumulator. A Broca galvanometer of 20 ohms resistance was used to balance the bridge.

The next step was to make sure that the main current altered by a known amount when reversed. This was accomplished, as in Nettleton's experiments, by means of a special circuit which could be shunted across the main apparatus or a dummy apparatus, alternatively (*see* diagram), the change of current in the experimental wire being equal to the current in this shunt circuit. The circuit was adjusted at this stage by altering the resistance, Y, until the fall in potential across a standard ohm, X, remained the same on reversal of the commutator key, K.

The balancing of the Thomson and Joulean heats was then attempted. The bolometer being again put into action and the galvanometer key being kept depressed permanently, the effect of reversing the main current was tried. The deflection of the galvanometer spot after about 15 secs. gave some idea of the want of balance, and by adjusting the shunt current to a suitable value a balance of sufficient accuracy could generally be obtained for the purposes of these experiments. A difference of 1 in 100 in the shunt current usually produced a small perceptible effect.

The temperature difference between the ends of the experimental wire, i.e., between the two copper discs, was determined from the readings of two copper-constantan thermocouples of which the calibrations were known. The hot junctions of these couples were soldered with silver to the faces of the respective copper discs in contact with the heaters, the wires being brought out through horizontal slots and insulated with strips of mica. The thermo-electric E.M.F.s were read on a Clark-Fisher potentiometer to the nearest 5 micro-volts—this accuracy being amply sufficient for the purpose in view. The mercury thermometers were also read as a check, but, of course, gave readings considerably lower at the higher temperatures owing to stem errors and partly to slight temperature gradients in the heaters themselves.

To find the resistance of the wire under test, the fall in potential between the copper discs was measured when the main current of 2·7 amps. traversed the experimental wire, the shunt current being broken for this purpose. This has the advantage that no change need be made in the arrangement and that the resistance is found under the actual conditions of the experiment. Owing to the Seebeck E.M.F.s at the junctions of the wire with the copper discs, the fall in potential has to be read for both directions of the main current. It is easy to see that the mean of the two readings gives the fall in potential due to the resistance of the wire. The value of the main current was read off from a Weston millivoltmeter with suitable shunt, the accuracy obtainable being about one in a thousand. The value of the shunt current was obtained by noting the fall in potential across the standard ohm, X, the reading of a Weston milliammeter being used in setting the potentiometer.

DISCUSSION OF RESULTS.

The Thomson effect was measured throughout the range 40-300°C. in Armco iron and in three carbon steels containing respectively 0·15, 0·60, and 1·1 per cent. of carbon. Before starting the experiments each of the specimens was annealed from about 700°C. Measurements were also made on a specimen of pure annealed electrolytic copper, for a purpose to be mentioned hereafter. Some preliminary measurements were also made on hard drawn Armco iron between the temperatures 40°C. and 180°C., but were not considered so reliable as those mentioned above.

Each of the specimens was used in the form of wire 0·122 cm. in diameter, and except in the case of copper the experimental length was 4·5 cm. The copper wire had an experimental length of 6·0 cm., partly to bring the shunt current within a reasonable value by increasing the resistance of the specimen, and partly as a greater sensitivity is practicable owing to the high heat conductivity and consequent freedom from temperature storms. The results with copper fully justify this alteration.

A fact of considerable interest and importance became apparent as the experiments progressed. This arose from the discovery that the reversal of the heaters (i.e., making the heater at the lower temperature in one set of experiments, that at the higher temperature in another set) gave a systematically different value of the Thomson effect. This was not due, as subsequent tests showed, either to any inaccuracy of the thermo-electric readings or to any asymmetry in the apparatus due, for example, to leakage. It was, in fact, due to a real asymmetry in the specimen itself induced probably by inhomogeneity. On reversal of the heaters in the case of the higher carbon steels, the difference was observed to be greater, a result considered confirmatory of the inhomogeneity theory. Indeed so great was this effect in the 1·1 per cent. carbon steel, amounting to 50 per cent. of the mean at 60°C., that it seemed very questionable if taking a mean of the two results would give a reliable value of the true Thomson effect. However, as the asymmetry decreased very considerably at higher temperatures being only 2 per cent. at 300°C., it seemed reasonable until the point was further investigated to take the mean of the results found on reversing the heaters. It is significant that the experiments with copper showed no sign whatever of this asymmetry.

The experimental results for iron and copper are given in Tables I and II, and some of the results are shown graphically in Figs. 3 and 4. Fig. 4 shows the mean, results, on the same scale, for each of the five specimens.

TABLE 1.—*Armco Iron.* (*Annealed.*)

i	P.D.	$i_1 - i_2$	θ_A	θ_B	$\theta = \frac{\theta_A + \theta_B}{2}$	σ_θ
2·740 amp.	0·02337 volt	0·1026 amp.	184·4°C.	146·9°C.	165·7°C.	$-5{\cdot}58 \times 10^{-6}$
2·743	0·02127	0·1184	161·9	118·7	140·3	5·09
2·735	0·02543	0·1107	212·0	169·9	191·0	5·86
2·725	0·03129	0·0913	275·3	234·3	254·8	6·12
2·715	0·03308	0·0888	296·6	254·1	275·4	6·10
2·770	0·02695	0·1100	226·9	183·7	205·3	5·93
2·765	0·02864	0·1100	247·9	202·4	225·2	6·00
2·765	0·03618	0·0774	316·4	275·8	296·1	5·97
2·750	0·03894	0·0684	345·0	303·5	324·3	5·58
2·760	0·03726	0·0728	327·6	287·4	307·5	5·86
2·755	0·03480	0·1016	261·7	311·0	286·4	6·23
2·750	0·03030	0·1086	219·4	264·1	241·8	6·39
2·750	0·03314	0·1009	247·7	294·0	270·9	6·27
2·745	0·03062	0·1063	223·7	268·2	246·0	6·38
2·744	0·02768	0·1052	194·9	234·8	214·9	6·36
2·735	0·02695	0·1049	187·4	227·2	207·3	6·20
2·715	0·03675	0·0858	286·2	332·4	309·3	6·02
2·720	0·03751	0·0789	293·3	338·0	315·7	5·82
2·770	0·03836	0·0783	294·0	338·8	316·4	5·80
2·775	0·03664	0·0863	278·3	323·0	300·7	6·10
2·780	0·02609	0·0964	174·1	209·4	191·8	6·14
2·780	0·02879	0·1040	201·4	241·9	221·7	6·36
2·775	0·02103	0·1127	113·5	152·8	133·2	5·20
2·775	0·02350	0·1190	142·5	184·1	163·3	5·80
2·768	0·02230	0·1136	129·9	169·0	149·5	5·60
2·760	0·01759	0·1071	70·7	107·8	89·3	4·40
2·757	0·01759	0·1060	70·8	107·8	89·3	4·37
2·752	0·01948	0·1129	96·4	135·0	115·7	4·95
2·745	0·01425	0·0920	24·0	60·3	42·2	3·15
2·740	0·01518	0·1016	37·6	76·0	56·8	3·51
2·725	0·01436	0·0976	65·7	24·6	45·2	2·99
2·745	0·01837	0·1118	120·6	80·8	100·7	4·50
2.757	0·01858	0·1167	124·0	82·0	103·0	4·48
2·760	0·01644	0·1177	95·1	51·1	73·1	3·81
2·755	0·02035	0·1164	147·1	105·2	126·2	4·92
2·745	0·02260	0·1084	174·2	135·3	154·8	5·49
2·745	0·02520	0·1053	205·8	166·2	186·0	5·85
2·780	0·02708	0·1047	223·3	182·7	203·0	6·00
2·765	0·02871	0·1024	242·9	201·6	222·3	6·17
2·750	0·03182	0·0936	277·4	235·9	256·7	6·24
2·735	0·03647	0·0687	320·8	284·1	302·5	5·97
						calories/ coulomb-degC.

TABLE 2.—*Copper.* (*Annealed.*)

I	P.D.	$I_1—I_2$	θ_A	θ_B	$\theta=\frac{\theta_A+\theta_B}{2}$	σ_θ
2·650 amp.	0·003035 volt	0·1086 amp.	27·0°C.	93·0°C.	60·0°C.	$+0{\cdot}450\times10^{-6}$
2·685	0·003280	0·0993	55·6	115·6	85·6	0·485
2·675	0·003515	0·0927	79·5	139·9	109·7	0·482
2·685	0·003708	0·0992	97·4	161·6	129·5	0·510
2·690	0·003900	0·1049	116·7	181·7	149·2	0·560
2·680	0·004133	0·0973	140·5	203·4	172·0	0·568
2·675	0·004430	0·0940	173·0	235·7	204·4	0·594
2·670	0·004723	0·0924	202·4	266·4	234·4	0·612
2·670	0·005050	0·0894	236·5	300·0	268·3	0·636
2·718	0·005405	0·0912	260·7	325·0	292·9	0·675
2·750	0·003700	0·1076	152·6	84·8	118·7	0·511
2·745	0·004493	0·1017	232·4	165·2	198·8	0·590
2·740	0·005415	0·0998	325·4	256·0	290·7	0·680

The first point which should be noticed is that the Thomson effect increased more or less systematically with the rise in the percentage of carbon in the steels. This, of course, is to be expected from the thermodynamic relation

$$T\frac{d^2E}{dT^2}=\sigma_A-\sigma_B \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where σ_A and σ_B are the Thomson coefficients for two metals A and B,

E=thermoelectric E.M.F. of a couple composed of these substances,

T=absolute temperature,

on account of the fact that $\frac{d^2E}{dT^2}$ increases when the percentage of carbon in the steel is increased (Belloc, Ann. de Chim. et de Phys., 1903).

The second point to which attention should be drawn is the sudden bend which takes place in the iron and steel curves about 200°C. In this connexion it is worthy of notice that various changes have been detected in other properties of iron near this temperature. For example, Thomson and Whitehead* have found a change in the electrical resistance of pure iron and in its thermoelectric power against platinum at 220°C.; a heat evolution is reported by Monius and Scott† near 200°C.; while Mayneord, working in Prof. Smith's laboratory, has shown that there is a falling-off in the magnetic permeability of iron in fields of about 0·07 gauss at a temperature of about 200°C. and ending about 300°C., these temperatures varying considerably with the heat treatment of the specimen. This latter observation is of special interest as the fall in permeability seems to coincide with the flat minima shown in the Thomson effect curves, while Prof. Lea has shown‡ that Armco iron has a maximum tensile strength at 230°C. The flatness of the minimum between 210°C. and 290°C. is very noticeable in the steel curves, but it would be rash to conclude that this is due to the presence of carbon. It has been suspected, however, that Cementite does not lose all its magnetic properties at 215°C., and that some gradual change may take place throughout this region.

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* Proc. Roy. Soc., Vol. 120, p. 587, Feb. (1923).

† Chem and Met. Eng., 1069 (1920).

‡ Engineering, 30, p. 829, June (1922).

COMPARISON WITH OTHER THERMOELECTRIC DATA.

According to the thermodynamic treatment, first applied to thermoelectric phenomena by Lord Kelvin, and which lead to his discovery of the effect which bears his name, equation (2) should stand the test of experimental verification. The data afforded by the present measurements on iron and copper and by the numerous researches which have been made on the thermoelectric powers of these

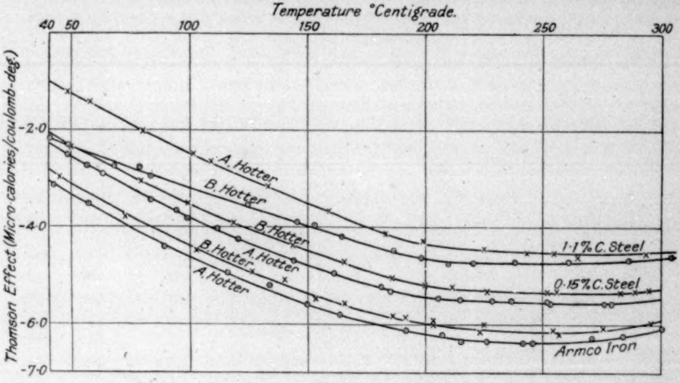

FIG. 3.

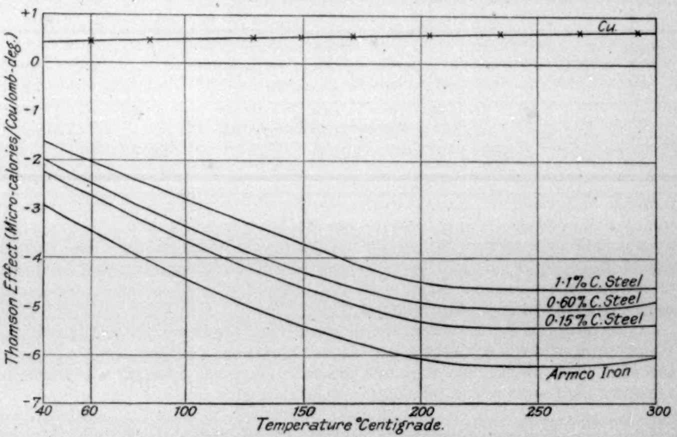

FIG. 4.

two metals at different temperatures provide a means of testing the equation. One of the most valuable researches is that of E. P. Harrison (Phil. Mag. III, p. 177, 1902), who has investigated the thermoelectric force of iron against copper over the range −200°C. to 1,000°C. Broniewski, in addition (Comptes Rendus, 156, p. 1983, 1913), has investigated the thermoelectric properties of the carbon steels.

In Fig. 5, the values of $\frac{\sigma_{Fe}-\sigma_{Cu}}{T}$, obtained from the present measurements, are plotted against the temperature, and Harrison's and Broniewski's results for $\frac{d^2E}{dT^2}$ are shown on the same diagram. Recognising the difficulty of measuring both the Thomson effect and $\frac{d^2E}{dT^2}$ accurately, in absolute units, it may be said that, on the whole, there is an approximate agreement between the Thomson effect measurements of the author and the thermoelectric power measurements of other workers. Nevertheless, further experimental confirmation of equation (2) was considered very desirable. As the apparatus for measuring the thermoelectric power is very easily set up, and as the samples of iron and copper were available from which were cut the specimens used in the Thomson effect measurements, some experiments were carried out on the thermoelectric power of these two metals. One of the copper-constantan thermocouples used in the previous research was taken to measure the temperature intervals, thus ensuring the same temperature scale as in the Thomson effect experiments.

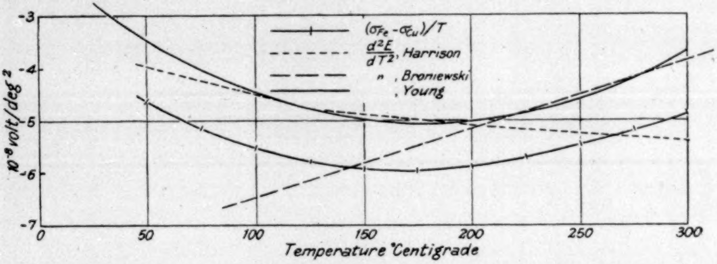

FIG. 5.

The values of the thermoelectric power in volts/deg. were plotted against the temperature, and the resulting curve showed more than a suggestion of a point of inflexion about 180°C. By trial, the following empirical equation was found to represent the results within the limits of experimental error :—

$$\frac{dE}{dt}=11{\cdot}15\times10^{-6}-2{\cdot}00\times10^{-8}t-1{\cdot}70\times10^{-10}t^2+0{\cdot}315\times10^{-12}t^3 \quad . \ . \ . \quad (3)$$

giving

$$\frac{d^2E}{dt^2}=-2{\cdot}00\times10^{-8}-3{\cdot}40\times10^{-10}t+0{\cdot}945\times10^{-12}t^3 \quad . \ . \ . \ . \ . \ . \ . \quad (4)$$

The latter equation gives a minimum value of $\frac{d^2E}{dT^2}$ at 180°C., the corresponding minimum in the curve derived from the Thomson effect measurements being at 178°C. This agreement is in itself striking, and is further confirmed by the parallelism of the two curves shown in Fig. 5, but there is, nevertheless, one serious disagreement between the two results. The Thomson effect measurements, while exhibiting the same general characteristics as those derived from the thermoelectric power, are numerically greater by an amount which is well outside the limits of experimental error. Whether this is to be ascribed to some omission in the theory of the measurements, or to an actual negation of the truth of the thermodynamic relation, is a matter for further consideration and experiment. It is significant, however, that constantan (the results for the Thomson effect in constantan were obtained by Dr. Nettleton) also shows the same peculiarity, and it is hoped to carry out shortly some experiments on nickel which may also confirm this observation. In addition, it may be noted that Latimer* has attempted to verify equation (2) from specific heat data, and while obtaining results which agree to 5 or 10 per cent. for most metals, finds considerable discrepancies for the ferromagnetic metals, that for iron being of the same order and sign as that in the author's investigations.

It seems possible that a Righi-Leduc effect may partly account for such a discrepancy, as in a soft iron wire the induction due to the current itself may be considerable, and would give an effect of the correct sign. It is difficult, however, to calculate the change in temperature of the bolometer coil which may be ascribed to this cause, and in consequence to determine the discrepancy, but an estimate gave a result only one quarter of that observed. The author's own opinion is that the similarity in the character of the Thomson effect and thermoelectric results points rather to the existence of some modifying cause such as that suggested above, than to an absolute denial of the irreversibility of the thermoelectric circuit. It seems quite possible that the Thomson coefficient may depend on the current, and, therefore, that the comparison of the thermoelectric power measurements which are made for zero current, with those of the Thomson effect which have been made for a current of 2·7 amp., is not justifiable.

In conclusion, I should like to express my indebtedness to Prof. Smith, not only for allowing me the privilege of working in his laboratory and placing apparatus at my disposal, but also for his kindly advice and encouragement during the progress of this work. I also wish to thank Dr. H. R. Nettleton, of Birkbeck College, London, for his courtesy in allowing me to see his apparatus under working conditions, and for the trouble which he took in explaining his methods.

Note on an Asymmetrical Heating Effect in Steel Wires.

[*Added 27th February*, 1925.]

In this note it is proposed to give a short account of the nature of the asymmetrical effect observed in the steel wires used in the preceding measurements of the Thomson coefficient and to put forward a probable explanation of the phenomenon.

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* Jour. Amer. Chem. Soc., 44, p. 2136 (1922).

In the first place, the phenomenon may be observed when the two ends of the wire are at the same temperature and when the temperature gradient in the wire is produced by the electrical current alone. If, under this condition, the electrical current is accurately reversed, an alteration in the temperature of the centrally wound bolometer coil is observed. Measurements of the potential difference between the ends of the wire were made for both directions of the current, and these were found to differ by less than one in a thousand. Yet to balance the asymmetry a current of 2·7 amp. had to be altered by 0·03 amp., which corresponds to an alteration of the heating effect of 2·2 per cent. Very careful tests were made to ensure accurate reversal of the current, and every precaution was taken to prevent leakage. The wire was also reversed longitudinally with respect to the rest of the apparatus, in order to make quite certain that the asymmetry was a property of the wire and not of the apparatus. The wire was also tested at various positions along its length, and although a considerable variation in the magnitude of the effect was observed, yet nowhere did any single wire show contrary effects in its different parts.

The results of these tests proved undoubtedly the reality of the phenomenon, and suggested that it could only be produced by an alteration in the radial temperature gradient on reversal of the current. Thus, for one direction of the current, heat is apparently produced near the surface and absorbed in the interior, while on reversal heat is absorbed at the surface and evolved in the interior. The ultimate reason why the effect is observed at all is because heat produced near the surface of the wire has a better chance of being conducted into the detecting coil and less chance of escaping along the wire than heat produced in the interior.

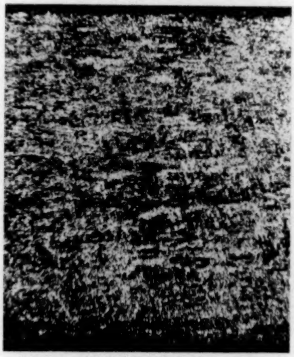
The variation of the phenomenon for different regions along the wire also suggested that its cause would probably be found connected with inhomogeneity of structure. This led to a photomicrographical examination of longitudinal sections of the wire, and it was soon discovered, as had been previously suspected, that there was a small but marked asymmetry in the structure as revealed by this means. An inspection of the accompanying photomicrograph, which shows the whole thickness of the wire (X 50), reveals the interesting result that, while the longitudinal markings near the top edge are on the whole inclined to the axis at a small angle, those near the bottom edge are inclined in the opposite direction. This gives to the section a "herring-bone" structure which, though not strikingly conspicuous, is nevertheless quite apparent when a careful examination is made. A number of measurements of the inclination gave an average value of about 2° near the edge. The structure is evidently related to the direction in which the wire is drawn.

Now, when a current passes through a wire there will be Peltier effects at the surfaces of the laminar constituents, of which the wire is composed, and if the thicknesses of the laminæ are unequal it is very easy to see that a "herring-bone" structure will cause the surface of the wire to be cooled, while the centre is heated, or vice versa. It remains to establish the fact that the Peltier effects required to produce the phenomenon are of a reasonable magnitude.

Let I = electrical current.

a = radius of the wire.

d = distance at which two adjacent laminæ of the same kind come to the surface.



LONGITUDINAL SECTION OF 0·71 PER CENT. CARBON STEEL WIRE (×50), SHOWING ASYMMETRICAL STRUCTURE.

To face page **154.**]

Let pd=length of surface occupied by one of the thinner laminæ.

P=Peltier coefficient.

θ=angle of asymmetry.

Let heat evolved per unit length of surface of wire per unit time=H.

$$H=\frac{2p\sin\theta}{a}IP$$

A corresponding quantity of heat will be absorbed throughout the interior of the wire, and I^2R will be evolved due to Joulean heating, where R=resistance per unit length.

Let nH of the heat produced at the surface and mH of that produced throughout the interior reach the coil.

Then heat supplied to coil per sec. per unit length

$$=nH-mH+mI^2R.$$

Hence, for constancy of temperature in the coil, we must have

$$(n-m)\frac{2p\sin\theta}{a}PI_1+mI_1{}^2R=-(n-m)\frac{2p\sin\theta}{a}PI_2+mI_2{}^2R$$

From which we get

$$\frac{dE}{dT}=\frac{(I_2-I_1)mRa}{(n-m)2p\sin\theta\,.\,T}$$

Now, n will be greater than m.

Taking the values $n=2m$, $\theta=2°$, $I_2-I_1=0{\cdot}03$ amp.

$p=0{\cdot}33$, $T=313$, $R=0{\cdot}00204$ ohm/cm.

we get

$$\frac{dE}{dT}=0{\cdot}50\times10^{-6}\text{ volt/deg.}$$

This corresponds to a difference of about 0·1 per cent. in the amount of carbon in the two constituents. This calculation shows that the Peltier effects produced in the wire are quite sufficient to cause an effect of the order observed.

There are two further pieces of evidence, however, which show that this theory is almost certainly the correct one. In the first place, some information can be obtained from the way in which the bolometer coil reacts to the reversal of the current when the asymmetry has been balanced. The galvanometer key being kept depressed, the reversal of the current causes an immediate deflection in the direction of the asymmetry, which is afterwards compensated by the Joulean heat. This shows that the effect which causes the alteration in the temperature of the wire must take place at the surface of the wire.

In the second place, it will be observed that the effect decreases more or less uniformly with temperature until at a temperature of 300°C. it is only one tenth of its value at 40°C. This is precisely the way in which the Peltier effect between two steels of different carbon content varies with temperature.

Incidentally, it rules out the possibility that the asymmetry is caused by Peltier effects at the points where the wire is fastened to the copper heaters because the

Peltier effect becomes zero at a temperature of about 260°C., and will consequently show reversal at 300°C. No such reversal of the asymmetry occurs.

Only a very few experiments were made to determine the relation between the asymmetry and the direction in which the wire was drawn. These experiments showed that if the wire was drawn through the die from right to left (thus producing a conical structure in which the apices of the cones pointed to the left) a heating effect was observed when the current passed in the same direction, and a cooling effect when it passed in the opposite direction.

In addition, it was found that the greatest effect took place in those portions of the wire which were last to pass through the die in the process of drawing. It will be easily seen that when such a wire is clamped between two copper discs the Peltier heat at the junction between the disc and that end of the wire in which asymmetry is greatest is of exactly the opposite sign to the asymmetrical heating effect. This was found to be the case when tested experimentally.

These experiments confirm the theory which is put forward in this note as to the nature of the asymmetry, and they clearly rule out the possibility of their origin arising from Peltier heat produced at the ends of the wire.

DISCUSSION.

Dr. D. **Owen** said that he would like to see very large numbers of experimental results of the kind obtained by the author, as those contained in the Paper were not numerous enough to support any one theoretical explanation exclusively. The results might, for instance, depend on the geometrical form of the specimens.

Dr. L. **Simons**: Throughout his work upon which that of the present author was based. Dr. Nettleton was fully alive to the difficulties involved in the heterogeneity of an ordinary drawn metallic wire. Would it not be possible to prepare a large single crystal of a pure metal, carefully cut in a definite direction with regard to a crystallographic axis, and then use the specimen for the determination of the Thomson coefficient ?

Prof. F. L. **Hopwood** said that if the asymmetry detected by the author was to be explained by the theory he had put forward (depending on the structure of the wire), it might be expected that the effect would be emphasised by torsion. Had he noticed any hysteresis effect ?

Prof. C. V. **Boys** suggested that the author should draw his own wires and note the relation between his asymmetrical results and the direction of drawing.

Mr. F. E. **Smith** suggested that the wire manufacturers would no doubt be willing to supply specimens labelled with information as to the direction of drawing.

Dr. H. R. **Nettleton** (communicated) : I should like to congratulate Mr. James Young on the successful way in which he has carried out his measurements on the Thomson effect. Many points in his Paper interest me, and I should like to mention one or two. I note that in his later work Mr. Young has been using as his "isothermal equipotential ends" split discs which are clamped together. I am at present engaged in attempting to measure the Thomson effect in wires under tension, and I have long abandoned the use of solder and am using rectangular copper blocks, which are pressed together and force the wire into a cavity of small diameter. Like Mr. Young, I find this most satisfactory, my only difficulty arising when the wire thins under heavy strain. I am pleased also to observe that Mr. Young is using a compensating coil. Recent experience has taught me that the best compensating arrangement consists of a single turn wound on the experimental wire alongside the main detecting coil, all four leads being brought out side by side, embedded in wool, to their terminals. Mr. Young's method of measuring the resistance under the very current used for the reversals (instead of a current of 1·0183 amperes) saves a vast amount of time. It would seem, too, on the face of it, to be theoretically better, but I have experienced mathematical troubles in attempting to prove it rigorously. I should, nevertheless, use it myself if my potentiometer enabled me to read easily above 30 millivolts. I should like to know if Mr. Young's resistance measurements, when plotted against mean temperature, show any sign of bend in the neighbourhood of 220°C. I am interested in Mr. Young's remarks on heterogeneity which is doubtless the greatest obstacle he has encountered in his research. There is very little reliable information on the absolute value of the Thomson effect, and I hope that Mr. Young will be able to continue his valuable research work.

AUTHOR'S REPLY :—(To Dr. Owen.) The reason for examining the Thomson effect in steels is given in the introductory paragraph of my Paper. Although I recognise the importance of confining attention to pure metals, I would point out that the properties of mixtures are not undeserving of attention. The very interesting problem of the asymmetrical effect in the steel wires, for example, arises out of such experiments. I have not yet tried sufficient experiments for the purpose of finding how the asymmetry varies with the length and diameter of the specimen, to give any definite results on that point.

(To Prof. Hopwood.) A certain amount of temperature hysteresis has been detected in the experiments with iron, the Thomson effect being found to vary slightly with the heat treatment of the specimen. With the steels, however, this effect was very much less marked.

(To Prof. Boys.) I regret I am in the position of not having drawn my own wires. I undertook the research at the suggestion of Prof. S. W. J. Smith, who very kindly provided me with the wires from some of his own specimens.

(To Dr. Nettleton.) I have to thank Dr. Nettleton for his remarks. I have examined my resistance measurements, and find no trace of any sudden change in curvature in the "mean temperature-resistance" curve for iron. There are, of course, small and sudden changes in the resistance of iron at various temperatures, one of which is referred to in my Paper; but the effect of measuring the resistance of a wire under a temperature gradient and plotting this against the mean temperature would be to smooth out any irregularity of this kind.

XIV.—IMPROVED CATHODE-RAY TUBE METHOD FOR THE HARMONIC COMPARISON OF FREQUENCIES.

By D. W. Dye, *B.Sc.*

Received February 11, 1925.

Abstract.

Convenient arrangements for the superposition of a telephonic and a radio-frequency displacement of the light spot of a Cathode-Ray Tube are described. The arrangements include the following :—

1. Circular or elliptical trace at a telephonic frequency to serve as a time axis. When great steadiness and uniformity are desired, a valve-maintained tuning fork controls the rate of revolution of the light spot.

2. Radial, circular and straight-line displacements at low radio or high audio-frequencies are next described and examples of the stationary patterns produced when these are superposed on the circular or elliptical time trace are given.

3. A special application of the method to the delineation of wave forms consists in the superposition of a long narrow elliptical time displacement and a straight-line high-frequency displacement in a direction at right angles to the major axis of the ellipse. By this means a considerable portion of the time ellipse is nearly a straight line representing a uniform time axis. The wave shape at the high frequency then appears in normal form and may be made of quite considerable size.

Some examples of each of the foregoing methods are given in the form of reproductions from prints made directly on pieces of bromide paper held down on the top of the Cathode-Ray Tube for a few seconds (3 to 10).

A special case occurs when an oscillatory valve system is arranged with grid and anode circuits closely coupled and harmonically resonant to each other. Wave forms consisting of a fundamental and a resonant single harmonic can be produced in this manner. Examples of current wave shapes in the grid and anode circuits are given, together with the voltage wave shape induced in a loosely coupled untuned coil.

THE cathode-ray tube has been used by a number of investigators for the delineation of wave forms and the determination of harmonic ratios of radio frequencies.

Various methods have been adopted to provide a convenient time base and to arrange so that confusion of the trace obtained is avoided.

Of the methods adopted, mention may first be made of the use of Lissajous* figures for the harmonic comparison of two radio frequencies. The chief limitation of this method is that ratios greater than about 15 : 1 introduce such complicated patterns that their interpretation and photography is difficult.

A method worked out in great detail by Dufour† consists in producing a zig-zag time trace across and down the screen. The high-frequency wave is superposed upon this in a direction at right angles to the time trace. For certain kinds of work this method is valuable, and allows results to be obtained which otherwise would be impossible. The equipment is very elaborate and expensive, and does not conveniently permit of a stationary pattern being formed. It is, therefore, necessary to take a photographic impression directly upon the plate inside the tube.

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* Lissajous Figures. F. Braun, Wied. Ann., 60, p. 552 (1897) ; and E.T.Z., 19, p. 204 (1898). Simon and Reich, Phys. Zeit., 2, p. 285 (1901). "Primary Radio Frequency Standardization by Cathode-Ray Oscillograph," Science Paper No. 489, Bureau of Standards (1924).

† A. Dufour, "Cathode-Ray Oscillograph : A Modified Braun Tube Arrangement," Jour. de Phys. et le Radium, 1, p. 147 (1920).

This latter necessity is, of course, inseparable from any method in which photography of a transient or non-recurring phenomena is required. In many cases, however, the oscillations under measurement or investigation are steadily maintained. It then becomes possible to produce a stationary pattern by arranging so that the path followed by the ray is repeated after a short time interval.

More recently Kipping* has used a neon tube trigger arrangement for producing a time trace which possesses some advantages and is very simple. In Kipping's Paper the idea of using the rotating ray method about to be described is suggested amongst others, but the idea originated a considerable time before with the present writer, and was successfully used at the National Physical Laboratory about 18 months ago. As long ago as 1899 Zenneck used a rotating ray for low-frequency measurement. E. Lübke† has also used a rotating ray for the purpose of delineating wave shapes at high frequencies ; in this method the exact analogy of the point by point method is used. The circularly rotating ray acts as a switching device by first passing through a radial slit, and thence between a pair of plates. The instantaneous conduction thus provided between the plates allows a voltmeter or electrometer to become charged to the corresponding instantaneous voltage. By slowly rotating, a few degrees at a time, the system producing the circularly rotating ray, the unknown wave-shape may be plotted out. The method of course, requires the special attachments within the tube to enable the system to work. The tube was of the heated cathode type, and operated with 400 volts anode potential.

The rotating ray method—which does not seem to have received much attention—can, however, be used with great advantages in many cases. In particular the method is valuable for the purpose of setting a low radio frequency to an exact value, for purposes of calibration, &c. The method consists simply in producing a circular or elliptical time trace by means of two-phase voltage of constant and low frequency.

The radio frequency may then be caused to operate on the ray in one of three different ways. These are as follows :—

1. A small voltage (about 20-30 volts) at the radio frequency is introduced into the steady anode voltage supply to the cathode-ray tube. By this means the ray receives a radial vibratory displacement due to the radio frequency, at whatever position it may be in its circular or elliptical path at the audio frequency. This is due, of course, to the variation in velocity of the electrons forming the ray, consequent upon the variation of anode voltage.

2. A small circular movement may be imparted to the ray at the radio frequency whilst the large circular or elliptical movement is provided by the audio frequency.

3. A vibratory movement, in a fixed direction, may be given by the radio frequency, whilst the large circular or elliptical movement takes place at the audio frequency.

The choice of these three methods is determined by the particular object in view in any given case.

Convenient circuit arrangements for carrying out these three methods have been worked out to suit the Western Electric Tube with heated cathode, and are described below after a short account of the best means of producing the rotating ray.

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* M. V. Kipping, " A Practical Demonstration of Some Applications of the Cathode-Ray Oscillograph," Wireless World, p. 705, Mar. 5 (1924).

† E. Lübke, " Measurements of Wave Form with Braun (Hot Cathode) Tube," Archiv. für Elektrot., 6, p. 161 (1917). Translation by writer, Electrician, 83, p. 270 (1919).

The most accurate method of producing the circular time base is to use a valve-maintained tuning fork as a fundamental time standard. A small voltage obtained from the fork is amplified up by a two-stage amplifier, so as to provide a source of voltage of very constant frequency. This voltage is converted to a two-phase source by the help of a condenser and a virtual non-inductive resistance. These two voltages of adjustable amplitude and phase relationship are applied to the two deflecting plate systems of the cathode-ray tube.

A convenient set up for the method is shown in Fig. 1 in diagrammatic form.

The tuning fork is shown at A, and is of ordinary form. The small voltage given by the output winding will not in general be of good wave form. In order to improve this the anode circuit of the second valve of the amplifier is a tuned circuit, consisting of the air core self-inductance L_1 and adjustable condenser C_1. The coil L_1 is best made with a generous amount of wire, so as to be of fairly good time constant, with inductance of about 0·5 henry. A secondary winding closely coupled to L_1 provides the supply of nearly sine wave form. This is connected to the phase-splitting arrangement whereby the two quadrature components are produced.

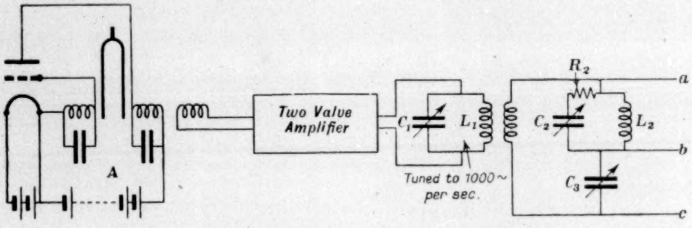
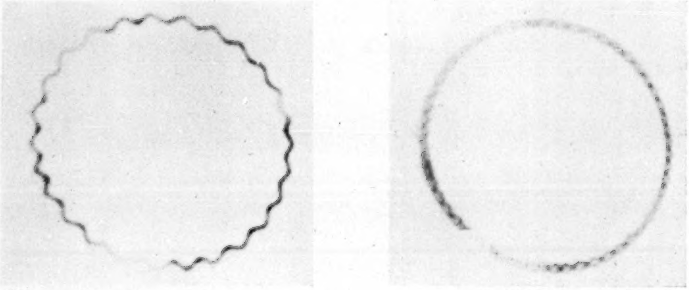


FIG. 1.—ARRANGEMENT FOR PRODUCING CIRCULAR TIME TRACE.

The phase splitting arrangement consists of the condenser C_3 in series with a tunable resonant circuit L_2, C_2. This circuit when adjusted to resonance behaves as a pure non-inductive resistance of value adjusted by the resistance R_2. It has been found desirable to use a virtual resistance of this form in preference to an actual resistance. The reason for this is that when an actual resistance of say, 10,000 ohms is used, this resistance has also a high impedance at the radio frequency. It then becomes almost impossible to keep a small parasitic radio frequency voltage from finding its way on to the deflecting plates connected to this resistance. By the device indicated a truly non-inductive resistance is provided at the audio frequency, whilst at any other frequency not very near the resonant one the impedance is very low.

The two quadrature voltages are applied to the deflecting plates, and by adjustment of C_2 and R_2, a nearly perfect circle is obtained after tuning C_1 to resonance.

The circle is adjusted to any desired size by adjustment of the brightness of the filament of the second valve of the amplifier. It can, of course, be made an ellipse



(*a*) Radial high frequency deflection of 26,000 cycles per second on a circular time base at 1,000 cycles per second.

(*b*) Radial high frequency deflection at 26,500 cycles per second on a circular time base at 1,000 cycles per second.

FIG. 4.

[*To face page* 161

of any eccentricity by slight detuning of L_2, C_2, or by adjustment of R_2 or C_3. In cases where it is not important to know the radio frequency exactly, the tuning fork may, of course, be dispensed with, and a low-frequency oscillator used instead. The freedom for the choice of a suitable rotation frequency is then a great advantage in many cases.

The valve oscillator and coupling coils are as shown in Fig. 2. This, at first sight, appears to be of normal type, but the arrangement of the capacities and windings L_1, L_2 permits of the production of resonant harmonic wave forms as discussed later on. The coils A, B are for the purpose of applying the radio frequency voltage to the cathode-ray tube in a number of different ways to suit different requirements.

When required, coil B is connected to a pair of coils wound on a double ebonite bobbin of such form and proportions that they produce a uniform magnetic field which is transverse to the axis of the tube. These coils are shown in diagrammatic form in Fig. 3, at F_1, F_2, which indicates the general cathode tube arrangement. The bobbin carrying these coils is made a sliding fit on the parallel part of the tube, and can be rotated so that the direction of the magnetic field can be altered in azimuth.

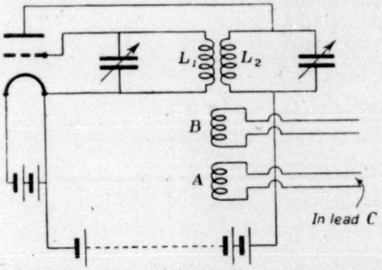


FIG. 2.—VALVE OSCILLATOR WITH RESONANT HARMONICS.

Coil A provides a radio frequency voltage which may be introduced either into the anode battery circuit of the cathode ray tube at " d " in Fig. 3, or in the circuit of one pair of deflecting plates at " e."

An example of the pattern produced by the superposition on the time circle of a frequency of 26,000 cycles per second introduced as a voltage in the anode circuit is shown in Fig. 4(a). This figure is reproduced from a photograph made by holding a piece of bromide paper down on the top of the cathode tube for about three seconds. The adjustment of frequency must, of course, be very precise in order that only a negligible drift occurs during three seconds. If, for example, a drift of one-tenth of a cycle is permissible during the three seconds, the constancy and steadiness of the high-frequency source must be equal to about two parts in a million. This can be secured by allowing everything time to settle down, and then by catching the picture just as it passes through a stationary condition corresponding to harmonic synchronism.

The value of frequency is given directly by counting round the number of waves in the circle and multiplying by 1,000. The method not only allows exact frequencies, such as 26,000 to be obtained, but a frequency such as 26,500 or 26,333 can also be set with perfect certainty. There is considerable difficulty in photographing these fractionally harmonic synchronous frequencies, since the light spot has to make two or three revolutions before retracing its path. A correspondingly longer exposure is necessary, and at the same time the permissible drift is reduced. A reproduction of such a case is shown in Fig. 4(*b*). There is, however, no difficulty in setting a frequency to such values; in this manner a considerable number of standard reference frequencies is available for calibrating purposes.

One of the disadvantages of this method of superposition is that a rather high voltage at the radio frequency is necessary to produce the requisite radial deflection; a loss of definition occurs when the voltage reaches its minimum value. The radial movement permissible is also very limited.

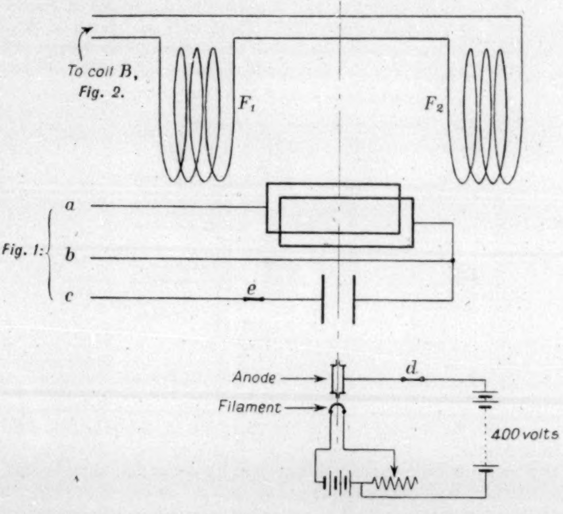
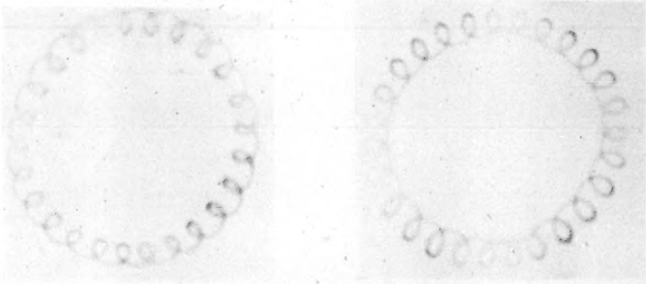


Fig. 3.—Cathode Ray Tube Arrangement.

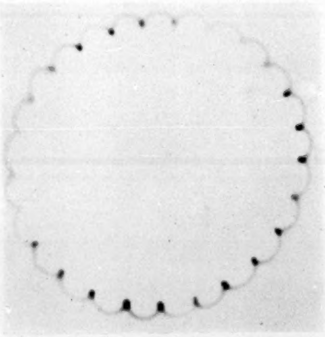
Superposition of Circular Motions.

The arrangement for this method can be seen from Fig. 2 and Fig. 3. The high frequency rotation of the light spot is produced by the help of the field coils F_1, F_2, previously described. These produce a magnetic deflection in a plane which can be rotated by turning the double bobbin about the axis of the tube. The quadrature deflection is produced by introducing the high-frequency voltage given by



(*a*) Circular high frequency at 26,000 cycles per second rotating in same direction at the circular time base of 1,000 cycles per second.

(*b*) The same as (*a*), but the two circular motions are in opposite directions.



(*c*) Conditions as at (*a*), but the amplitude of the high-frequency motion has been adjusted to give a cusp.

FIG. 5.

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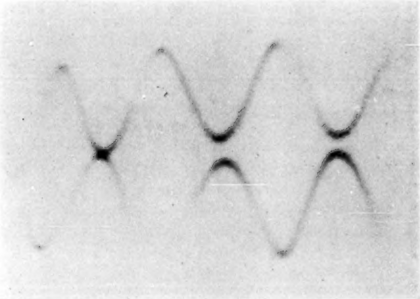
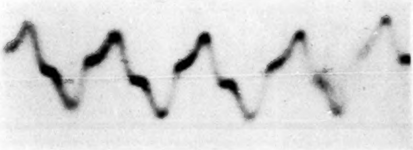


(*a*) Sine wave at a frequency of about 15,000 cycles per second. Long elliptical time base.



(*b*) Same frequency as (*a*), but with a considerable proportion of a third harmonic.

FIG. 6.

inducing coil A into the circuit at the point "e" of Fig. 3. This voltage can also be obtained from the ends of the deflecting coils F_1, F_2, by suitably choosing the inductance of these.

The phase adjustment is made by turning F_1, F_2, about the axis of the tube. The amplitude adjustment is made by sliding the coils F_1, F_2 up and down on the tube or by variation of coupling of the inducing coils A and B. The small high-frequency circle is obtained and adjusted with the large low-frequency circle cut off.

When both circular motions are allowed to operate simultaneously the beautiful circular looped patterns shown in Fig. 5 (*a*) and (*b*) are obtained. Two cases arise, the two circular motions may be in the same or in opposite directions. The corresponding patterns are shown in Fig. 5 (*a*) and (*b*), reproduced from photographic prints taken in the same manner as Fig. 4.

An inspection of the two pictures given in Fig. 5 shows that for the same high frequency the number of loops is two greater in one case than in the other. When the two circular motions are in the same direction, the loops are on the inside and the number of them is one less than the ratio of the frequencies. When the two circular motions are in opposite directions, the loops are on the outside and the number of them is one more than the ratio of the frequencies. By adjustment of the relative diameters of the two circular motions it is possible to obtain a sharp loop or cusp resulting from the instantaneous arrestment of the spot. This condition is very satisfactory photographically, and also renders the setting of fractional harmonics more certain. One of these latter is shown in Fig. 5(*c*) for a case corresponding to (*a*). Although it is well nigh impossible to photograph them, it is not difficult to set frequencies corresponding to seven or eight journeys of the light spot before the cycle is repeated. For example, a frequency such as 24,875 can be set by this means. The advantage of this method over the foregoing method is the much smaller high-frequency voltage necessary and the formation of loops rendering counting or photography easier.

The arrangement is, however, not convenient for the examination of wave forms. For this purpose, therefore, the following method (3) is recommended. This method is merely a particular case of (2) just described. One of the components of the high frequency is suppressed—by removal of the deflecting coils F_1, F_2, for example. The *h.f.* circular motion becomes a straight line of fixed direction. The *l.f.* circular motion is then made elliptical with the major axis perpendicular to the *h.f.* direction. By making the eccentricity considerable we can greatly open out the time scale, and also render it nearly a straight line for a considerable length. The time scale will, of course, not be uniform right round the ellipse, but if it is long and narrow there will be a portion in the middle of the nearly straight part of sufficient length and uniformity of velocity to be quite satisfactory for delineating wave shapes at moderately low radio frequencies without distortion. The ordinate motion at the radio frequency may be made large when this method is used, and a reasonable size of wave obtained.

Examples of a good wave and of a wave with harmonics are given in Fig. 6. The nature of the waves with harmonics and the special uses of such waves will follow later.

The applications of the foregoing methods to telephonic frequencies are obvious; it is merely necessary to use a slower time base corresponding to a frequency of revolution of, say, 100 ∼ per second, instead of 1,000 ∼. For quantitative work it is very desirable that the two motions should be proportional to their respective quantities; they should also be rectangular. For these reasons the high-frequency voltage should be introduced into the lead marked C on Fig. 3 at the point "e," and the elongation of the ellipse must then be obtained by reducing C_3 to about a quarter of its original value. The rectangularity of the high-frequency motion with regard to the major axis of the ellipse can be seen by eye when the two motions are taking place together, but at an inharmonic ratio. Adjustment is made by condenser C_2.

The limits of frequency which may be standardized are determined largely by the tube itself. Owing to the relatively low velocity of the ray, the time occupied by the electrons during passage between the deflecting plates is such that loss of definition occurs at frequencies above 200,000 per second.

This value cannot, however, be directly compared against a fundamental time base of $\frac{1}{1000}$ of a second owing to uncertainty of counting. This trouble can be entirely overcome by the use of resonant harmonics. A brief account of these is given in Section II. of the Paper.

SECTION II.

RESONANT HARMONICS IN VALVE OSCILLATORS.

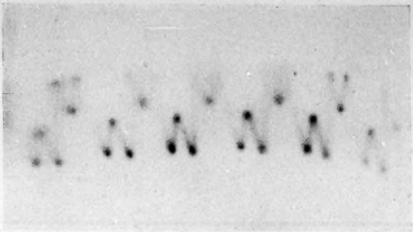
The distorted wave shown in Fig. 6 was known to be of a kind which commonly occurs in small valve oscillators such as heterodyne wavemeters and sources.

It is, of course, well known that curvature of the valve characteristics and changes, with amplitude, of the internal anode and grid impedances can, and indeed must, introduce harmonics into the wave shape produced, but in the case shown and in many other cases the distortion was so large that it was felt that other special causes were responsible for the distortion.

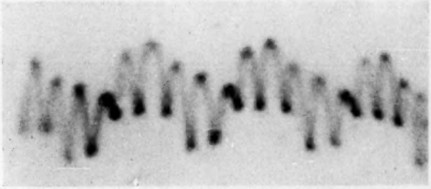
The matter was therefore followed up somewhat. Observations of a power-valve oscillator at the laboratory showed that when working in a normal manner the wave form was very good. The generator was such that normally the oscillatory circuit was of very small decrement and was in the anode circuit of the valve. The grid winding was of thin wire and had such mutual inductance to the oscillatory coil that the voltage on it was about one-fifth that on the oscillatory coil. The coupling between the coils was close. There was, in this case, no condenser across the grid winding. When, however, the main oscillatory circuit was in the grid circuit of the valve with the fine-fire coil as inductance whilst the heavy-wire coil was in the anode circuit, the distortion was very pronounced.

A small variable condenser was accordingly placed across the anode winding. The value of this condenser was slowly increased whilst at the same time the frequency was kept constant by a careful simultaneous adjustment of the grid oscillatory circuit condenser. A remarkable series of changes occurred in the wave shape thus held approximately steady on the oscillograph. Harmonics such as the ninth, eighth, seventh, sixth, fifth, &c., successively became reinforced and reached large values as the anode condenser was varied.

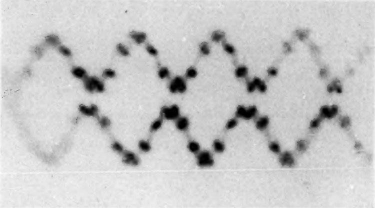
When a harmonic is at its maximum amplitude the wave shape appears to consist almost entirely of the fundamental and this harmonic; such cases are shown in Fig. 7, (*a*), (*b*), (*c*) and (*d*). At intermediate settings of the anode condenser the



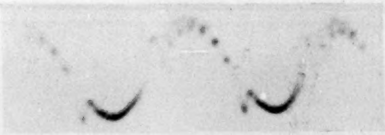
(*a*) Fundamental of 24,000 cycles per second with strong third harmonic.



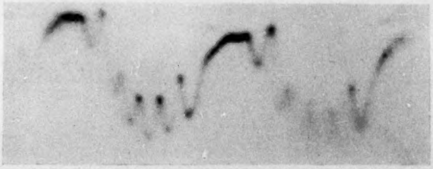
(*b*) Similar to (*a*), but with a resonant fifth harmonic.



(*c*) Fundamental of 25,000 cycles per second with resonant harmonic giving 175,000 cycles per second.



(*d*) Showing interference between two neighbouring odd and even harmonics.



(*e*) A case similar to (*d*) for the seventh and eighth harmonics of a fundamental of about 12,000 cycles per second.

FIG. 7.

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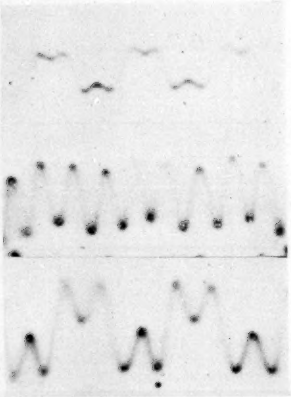

(*a*) Current in grid coil circuit of oscillator. Fundamental of about 20,000 cycles per second.

(*b*) Current in anode coil under exactly the same conditions as (*a*).

(*c*) Voltage induced in a coil loosely coupled to the oscillator.

FIG. 8.

[*To face page* **165**.

wave shape passes through curious forms. For example, Fig. 7 (*d*) shows a shape corresponding to about equal proportions of two adjacent harmonics and very little else. These two harmonics reinforce each other during one-half of the wave and cancel each other out during the other half. In some cases the harmonic oscillation does not build up and die down smoothly during each half-wave, but commences with a jerk as shown by the curious peak marked " *a*." A similar case for the seventh and eighth is shown in Fig. 7 (*e*).

These harmonic resonances correspond undoubtedly to a setting of the condenser of the anode oscillatory circuit such that the resonant frequency of this circuit bears an integral ratio to the main grid oscillatory circuit frequency. It is to be expected that the valve is able to produce the two corresponding frequencies simultaneously. It might be thought that two inharmonic frequencies could also be produced in the same manner, but such does not appear to be the case. The transition from one wave shape to another is smooth and continuous. It is not possible to obtain a stationary pattern and get a blurred figure at the same time, such as would occur if the two frequencies were inharmonic.

The wave shapes shown in Fig. 7 represent electromotive force induced in a coil coupled to the oscillator by mutual inductance. The voltage induced in such a case is, of course, proportional to " ω," so that the harmonic components in the wave of induced voltage will be magnified in proportion to the order of the harmonic by comparison with the corresponding components of the current wave in the oscillator. The current wave in a practical case will therefore in general be much smoother than the waves of voltage shown in Fig. 7. In many cases, however, we are directly concerned with the voltage induced in a circuit. In such cases the wave shapes shown are those which must be considered.

In the actual oscillator used in these experiments we have two closely coupled circuits both carrying currents of different wave shapes and both coupled to the external coil in which the wave shape of voltage is produced. The voltage wave will therefore depend upon the two current wave shapes and the values of the two mutual inductances to the external coil. The phase relationship of the two current waves will also largely affect the induced voltage wave shape.

In Fig. 8 are shown three wave shapes corresponding to one condition of the oscillator and its circuits.

The curves " *a* " and " *b* " are current wave shapes in the grid and anode coils respectively. Curve *c* is the voltage wave induced in a coil. The current waves were obtained by inserting the deflecting coils F_1 and F_2 directly into the circuit concerned. The coils were then oriented round until the deflection was at right angles to the major axis of the ellipse.

Unfortunately it was not possible to observe the phase relationship of the two currents. This could, however, have been done by the aid of two pairs of coils with axes at right angles to one another. The time circle would not then be used. A small Lissajous figure would be obtained in which a distorted ellipse would be formed by the two fundamental components each with its harmonic.

Referring to Fig. 8 again, the current waves produced are those which might have been expected. The anode circuit, of harmonic resonant frequency and small damping coefficient, has a current wave shape of which the main component is the harmonic, but in which there is a noticeable amount of fundamental present. The grid circuit of resonant frequency of the fundamental has a greater damping coefficient than the anode circuit. As a consequence the wave shape has a considerable proportion of the harmonic component present.

The voltage wave shape is similar to those shown in the previous examples given.

The writer is not aware that this particular property of a valve oscillator has been observed before. For certain purposes such an oscillation is of great value. For example, a cascade system of oscillators all in harmonic synchronism can be built up whereby a very high radio-frequency may be controlled from a lower one.

For standardization purposes the method is particularly suitable. One can, for example, use a fundamental frequency of, say, 30,000 cycles per second with a resonant ninth harmonic and so obtain a frequency of 270,000 cycles per second in a manner which leaves absolutely no uncertainty regarding frequency.

DISCUSSION.

Mr. Norman Kipping: A variety of the harmonic generator referred to in the Paper has for some years been in use in "Carrier" telephony ("Carrier Current Telephony and Telegraphy," E. H. Colpitts and D. A. Blackwell, Journ. A.I.E.E., Vol. 40, Nos. 5 and 6, May-June, 1921). In this case the most convenient arrangement has been found to be the use of an amplifying vacuum tube rather than an oscillating vacuum tube, for the introduction of harmonics. An oscillating vacuum tube would be used to generate, say, 5,000 cycles, the output being fed into an amplifying element in such a way as to overload it. For obtaining a supply of the various harmonics produced by any tube when overloaded (in this case 10,000, 15,000, 20,000 cycles, &c.) selective circuits would be employed. The original 5,000 cycle wave would also be amplified without distortion and transmitted along the transmission line. At the far end it would be introduced again into a distorting amplifier (by filters). Further selective circuits at the distant end would ensure that the carrier waves at the two ends of the transmission line were truly equal in frequency. Mr. Dye is to be congratulated very heartily on his Paper. His method of frequency calibration employing a rotating magnetic deflection is particularly clever. From the standpoint of wave-form examination Mr. Dye's method is less simple than my neon-lamp method mentioned in his Paper. The two systems have, however, about the same range. As Mr. Dye has referred in his Paper to my prior publication of the "cogged wheel" frequency calibration device, it may be stated that I was working on this quite independently. No doubt Mr. Dye was actually before me.

Mr. R. A. Watson Watt: I would join in congratulating the author on his extremely elegant method for the comparison of frequencies of high ratio and over wide ranges, particularly when the application of the looped circular pattern is supplemented by the "double graduation" provided by the resonant harmonic method. Mr. Dye will perhaps not disagree with me in thinking the applications to wave-form delineation less important and less elegant. In work on the wave-form of atmospherics, Prof. Appleton, Mr. J. F. Herd and I passed from the use of an elliptic base of zero minor axis to the elliptic base of moderate eccentricity recommended by Mr. Dye, but we were glad to abandon both in favour of a device which we called the "Linotime," and which I regard as offering, for similar limits of dimensions of pattern, intensity of illumination, &c., a much closer and more conveniently attained approximation to linearity than do the open ellipse of Mr. Dye's scheme or the exponential curve of Mr. Kipping's neon-tube method. The device, to whose development my contribution is negligible in comparison to those of my two collaborators, and on which patent applications have been filed, comprises a retroactive triode oscillator with a condenser in the grid circuit. The leak across the grid condenser takes the form of a saturated diode, and in the absence of other leak conductances the curve of recovery from the well-known phenomenon of negative charging of the grid, interrupting oscillation, is strictly linear, and is applied as a time base. The base thus obtained is both linear and unidirectional, since the recovery period can be made so long relative to the oscillating-charging period that the latter, providing the back stroke of the time base, fails to excite notable fluorescence over a wide range of base frequencies. The control of base period can, of course, be made as accurate as in other methods. The theoretical advantages of a system in which the inherently linear law is departed from only on account of slight defects in the apparatus, as against devices dependent on regarding inherently elliptic or exponential laws as sensibly linear over small ranges, are borne out in practice.

It is not clear from the Paper how Mr. Dye avoids the vitiating effects of synchronisation phenomena, which will in general, on account of the coupling within the oscillograph, produce a stationary pattern even when the independent frequencies under comparison depart from an integral ratio by more than the few parts in a million mentioned by the author. It would be of value if Mr. Dye would explain how he ensures that his measurement is made on the undisturbed frequency to be determined.

Mr. T. SMITH demurred to the author's terminology in the description of Fig. 5 of the Paper. He held that the number of loops is always equal to the number of the partial under investigation provided that the loops be properly counted, the complete passage round the circle being reckoned as one loop with positive or negative sign.

Dr. D. OWEN, in congratulating the author, said that the new utility he had given to this oscillograph opened up attractive vistas of experiment and greatly increased the value of the instrument. Was there any reason for limiting the fundamental frequency to 1,000, or would it be possible to start from, say, 25,000 ?

Prof. E. V. APPLETON (communicated): In connexion with the experiments on atmospheric wave-forms (Proc. Roy. Soc., A., 103, 84, 1923), it was very soon found that circular and elliptical time-bases were not nearly so useful as a linear and unidirectional base obtainable from a special type of triode oscillator circuit. By means of this oscillator (British patent application No. 3472/24), the cathode-ray spot can be made to travel from left to right uniformly in a controllable time, and then spring back to the origin almost instantaneously when the process is repeated. Vertical movements of the cathode spot are thus read off without distortion and without ambiguity as to time-sense. The electrical circuit used is an interesting example of a non-linear oscillator and the theory of its working, which depends on oscillation-hysteresis, has been fully worked out. An account of this will be published shortly.

This kind of time-base was developed in March and April, 1923, and has been in general use since then in all the stations carrying out wave-form observations for the Radio Research Board. It has also proved useful in experiments on the wave-form of the current pulses produced by the entry of α-particles into an ionization chamber (Proc. Camb. Phil. Soc., Vol. 22, p. 434, 1924). For the examination of periodic current or voltage changes the base is just as useful, since synchronization effects are usually sufficiently developed to keep the pattern stationary.

The " locking " of two oscillators, of which the frequency of one is n times the other, where n is integral, is well known, and has been used by Mercier for standardization purposes. The conditions under which this is possible are given in a Paper " On the Synchronization of Triode Oscillators " (Proc. Camb. Phil. Soc., Vol. 21, p. 231, 1922), where the formula (8) developed for the " locking " of fundamental periods applies equally well to harmonic synchronization.

AUTHOR'S reply (communicated) : The method of producing a resonant harmonic which I have described can, I think, hardly be classed with that referred to by Mr. Kipping. The method referred to by Mr. Kipping is, of course, well known, and has been used in principle for calibrating wavemeters for a long time, consisting, as it does, of the principle of first distorting a wave and then selecting the desired harmonic introduced by the distortion. In the method I have described, however, the two frequencies are simultaneously produced by one valve with only two oscillatory circuits and no other coils.

The method of delineating wave forms described in the Paper is certainly less simple than that using a neon tube, involving as it does the complication of an independent source and amplifying arrangements. Having, however, definitely required these parts for harmonic frequency determinations, it was considered of sufficient interest to show this further application of the apparatus.

I thank Mr. Watson Watt for his appreciative remarks and for his kindly criticism of the application of the method to the delineation of wave form. I am afraid, however, that, for the purpose in view, I must still maintain that the elliptical time base is much more satisfactory than any other form is likely to be. The particular point at issue in my use of the method is to determine the order of the resonant harmonic with regard to the main wave, and to be able to adjust so that any desired resonant harmonic within the range available is at its optimum value. It is necessary at the same time to adjust the fundamental high frequency so that it is an exact integral of the fundamental time base. I think it will be realised that for these conditions the method described is particularly suitable and satisfactory. But in cases of non-periodic phenomena such as atmospheric disturbances, and in many other cases, the method devised by Dr. Appleton is undoubtedly greatly superior.

With regard to the question of interlocking of the two frequencies, this, in the present case, is not a vitiating effect, but a desirable one. As a matter of fact, the effect is so small as to be negligible. The two frequencies will not interlock, even when one is different from a harmonic of the other by only one part in a million. The tuning fork has very great stability of frequency, and owing to the tuned circuit in the second stage of the amplifier, the wave forms of voltage applied to the cathode tube to produce the time circle are very nearly sinusoidal.

The radio-frequency source used in my experiments was operated on an anode potential of about 300 volts. It therefore possessed considerable stability of its own. This doubtless further contributed to the entire freedom from observable interlocking of the two systems.

Some of the points raised by Prof. Appleton have been dealt with in the reply to Mr. Watson Watts' remarks. The writer has read with great interest the Papers to which Prof. Appleton refers, and will look forward to the further Paper promised on the patented method of obtaining a uniform time trace, more especially in view of the fact that no hint of the method is given in either of the two Papers mentioned.

The author is, of course, quite familiar with the beautiful measurements made by Mercier on the absolute determination of the velocity of propagation of waves along parallel wires, in which a cascade system of oscillators all in harmonic synchronism is used. The present method, which only applies to frequencies within the limits of the oscillograph, does not make any use of the principle of harmonic interlocking of two frequencies, since the single-valve oscillator used cannot generate other than frequencies which are in harmonic ratio to one another.

Mr. T. Smith has called attention to a want of precision in regard to my explanation of the difference of two between the numbers of loops on the circles of Fig. 5 (*a* and *b*). I regret that in the reading of the Paper I did not express it in the same clear manner that Mr. Smith has done. Since, however, I have confined myself in the text to the mere statement of the results, no confusion of the reason can arise.

With regard to Dr. Owen's query as to the choice of a fundamental frequency ; there is, of course, no reason why any other frequency within the scope of the instrument should not be used. This is briefly referred to at the end of Section 1 of the Paper in regard to the use of a slower time base for telephonic work. The upper limitations are determined mainly by the limitations of the tube. For standardization purposes the choice of a time base of $\frac{1}{1000}$ sec. has been determined from considerations of accuracy and constancy. A suitably designed valve maintained tuning fork of a frequency of 1,000 periods per second is specially satisfactory from this point of view. Doubtless by the use of a piezo-electric quartz oscillator a sufficiently accurate time base of $\frac{1}{25\,000}$ second could be realised, but for the purposes of frequency standardization within the limits of the cathode-ray tube of the type used there would be no special advantage in this.

XV.—THE VARIATION OF YOUNG'S MODULUS AT HIGH TEMPERATURES.

By JAS. P. ANDREWS, *B.Sc.* (East London College).

Received December 19, 1924.

ABSTRACT.

The variation of Young's Modulus with temperature is found for zinc, silver, phosphor-bronze, lead, and soda glass by a static method, to within about 150° of the melting-point. For these substances (as for several others recorded) the modulus varies exponentially with temperature, so that q(Young's Modulus) $=q_1e^{-b_1t}$ (where q_1 and b_1 are constants) up to a temperature roughly half-way from absolute zero to the melting-point, and $q=q_2e^{-b_2t}$ for the remainder. The values of the constants are tabulated.

INTRODUCTION.

THIS Paper continues the work on Young's Modulus previously published,* in which the variation of the modulus of elasticity of nickel, platinum, aluminium, and silica was determined up to temperatures not greater than 900°C.

METHOD.

A rod or wire of the substance, about 5 cm. long, was suspended vertically from a clamp at its upper end, within an electric furnace. The whole could be rotated any angle in a vertical plane, and a knowledge of this angle, together with the observation of the deflection of the free end of the beam or wire (either under its own weight or under load) enabled Young's Modulus to be calculated at any temperature of the furnace.

APPARATUS.

The small electric furnace, which was previously closed with a copper end-piece, was during these experiments closed with a slab of uralite, to ensure a more uniform distribution of temperature. The slab was grooved to allow the passage of light through the apparatus, and the ends of the groove closed with glass windows.

A circular scale at the back of the furnace exhibited the angle of rotation, and upon it were stops to ensure exactly the same rotation on each occasion.

Fig. 1 shows the apparatus with the furnace rotated through 45°.

Temperature measurements were made with a small platinum resistance-thermometer, as shown in the diagram.

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* Lees, Andrews, and Shave, Proc. Phys. Soc., 36, p. 405 (1924).

THEORY.

Since the theory was given in the Paper already mentioned, it is only necessary to repeat the formulæ used on this occasion. These are :—

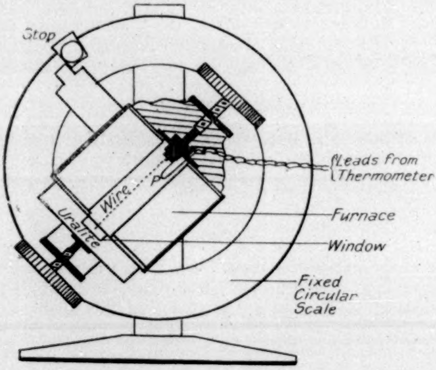
(1) For wires bending under their own weight,

$$(a)\ q=\frac{8gl^3m\cos\theta}{y_1d^4\pi}\quad\text{or}\quad(b)\ q=\frac{2\rho gl^4}{y_1d^2}\cdot\cos\theta$$

(2) When a weight is used whose centre of gravity is p from the free end of the wire,

$$q=\frac{64gl^2}{y_1d^4\pi}\left\{M\left(\frac{l}{3}+\frac{p}{2}\right)+m\left(\frac{l}{8}+\frac{p}{6}\right)\right\}^*$$

where q=Young's Modulus, ρ=density, g=the acceleration of gravity, l=length of the wire, d=its diameter, y_1=the deflection of the end measured perpendicularly to the length of the wire, m=mass of the wire, M=the mass attached to the end, and θ=the inclination of the wire to the horizontal.



P=Platinum Thermometer

FIG. 1.—DIAGRAM OF THE APPARATUS SHOWING ELEVATION WITH A SECTION OF THE FURNACE. THE OBSERVING MICROSCOPE WAS RIGIDLY CONNECTED WITH THE FURNACE, AND ROTATED WITH IT.

In several of the wires used, a measurable, progressive increase of length was noticed, taking place chiefly after viscous flow had set in. From time to time this increase was estimated, and a correction to the value of Young's Modulus obtained on the assumption that the volume remained constant, applied as follows :—

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* Owing to a misprint, the second term in the brackets, of this expression, appeared in the last Paper as $m\left(l+\frac{p}{6}\right)$.

From equation 1 (*a*) above

$$q=\frac{kl^3}{d^4} \text{ where } k=\frac{8\,gm\,.\cos\theta}{y_1\pi} \text{ a constant.}$$

But since the volume of the wire remains approximately constant,

$$d^2=\frac{\text{constant}}{l}$$

Hence $q=Kl^5$, where K is constant, and $dq=5Kl^4dl$,

$$\text{or } dq=\frac{5q}{l}dl$$

VISCOUS FLOW.

About halfway between absolute zero and the melting-point, the metals begin to exhibit viscous flow. Under these circumstances the deflection observed increases with the lapse of time. The calculation of Young's Modulus is therefore subject to some uncertainty unless the time taken in reading the deflections is very small. In the experiments the rotation of the furnace was rapidly accomplished, and the reading taken at the instant the rotation was complete, the end of the wire being kept in view the whole time. In spite of this, the last one or two readings must be regarded as doubtful in most cases. The probable correction required to Young's Modulus would be positive, thus raising the curve somewhat at highest temperatures.

RESULTS.

Zinc Wire (2).

Length, **4·847** cm., p. 0·1375 cm. Diameter, 0·0283. Used with weight M, 1375 gm.

$$\theta=70°. \quad q=\frac{7{\cdot}654\times10^{12}}{y}$$

Temperature.	y	$q\times10^{11}$	Remarks.
18°C.	8·95	8·85	
62	10·4	7·35	Very slight viscous flow.
98	11·0	6·96	
128	12·0	6·38	
150	13·5	5·68	
170	14·5	5·28	
198	16·7	4·58	
225	18·1	4·23	
260	20·1	3·82	Viscous flow very rapid.
288	18·9	4·05	
318	21·9	3·49	

This wire was subjected to a second heating, but on removal from the furnace it exhibited a tendency to collapse, bending sharply at one or two points along the wire. It appeared to be very soft.

Zinc wires exhibit a rapid descent of Young's Modulus which begins at room temperatures. From the general resemblance between this variation, and that at higher temperatures in other metals, it may be inferred that the rapid decrease in the case of zinc begins at some point below room temperatures. It may also be observed that the rate of decrease falls off slightly at the highest temperatures. This is in accord with observation on several other metals.

Silver Wire (3).

Length, 5·600 cm. Diameter, 79·78×0·000353 cm.

$\theta=20°$. $\rho=10{\cdot}5$. $q=\frac{1{\cdot}057\times10^{12}}{y}$

Temperature.	y	q	Remarks.
30°C.	1·45	$7{\cdot}28\times10^{11}$	
127	1·50	7·03	
195	1·52	6·95	
260	1·62	6·52	
324	1·65	6·39	
388	1·70	6·21	
448	1·90	5·56	
500	2·02	5·22	
560	2·50	4·22	Slight viscous flow.
619	3·10	3·40	
680	3·75	2·81	Viscous flow considerable.

This wire and one other which was tried (which had previously been heated to about 650°C.) showed small excrescences when removed from the furnace.

Young's Modulus for silver decreases slowly with increase of temperature up to temperatues roughly midway between absolute zero and the melting-point (995°C.), after which the decrease is much more rapid. A tendency is again shown, however, for this decrease to be somewhat retarded at the highest temperatures.

Lead Wire (3).

Typical of all lead wires tried. Length, 5·60 cm. Diameter, 114·84×0·000353 cm.

$\theta=69{\cdot}5$. $\rho=11{\cdot}37$. $q=\frac{6{\cdot}091\times10^{11}}{y}$

Temperature.	y	q	Remarks.
20°C.	2·72	$2{\cdot}24\times10^{11}$	Distinct viscous flow.
46	3·02	2·02	
70	3·70	1·65	
96	4·15	1·47	
120	4·60	1·33	
146	6·75	0·905	Viscous flow very rapid.
172	10·0	0·609	

The variation of Young's Modulus for lead appears to be almost linear over the range taken; but it must be remembered that, however rapidly the observations were taken, for the last two or three values, the viscous flow was so considerable that some error will certainly have been made. (See above, Viscous Flow.)

The apparent linearity was observed by Koch and Dieterle* also (who employed an oscillation method), but the above results differ widely from theirs.† In the range 0°C.—150°C. Young's Modulus for the rolled strips they used, decreased by 17·1 per cent., whereas the decrease in the above experiments amounts to more than 60 per cent. Part of this difference may be due to the very different previous treatment.*

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* Ann. d. Physik., 68, 5, No. 13, p. 441, Aug. 31 (1922).

† It may be pointed out here that Koch and Dieterle calculated their values by means of the formula $N=\frac{m^2}{4\sqrt{U3}}\frac{a}{l^2}\sqrt{\frac{E}{S}}$ (quoted from Kohlrausch, Lehrb. d. Prakt. Phy., Abschnitt, 54a), which is obviously applicable only to undamped vibrations. The effect of friction will be to diminish N, the frequency, and the values given by Koch and Dieterle seem to need considerable correction. The tendency would be to diminish the variation they record for the elasticity.

It may be noticed that in this case, the elasticity after heating appeared to be slightly less than before. This need not be actually the case, as it is more than probable that a slight stretching of the wire took place while it was in the very soft condition.

Phosphor Bronze Wire (1).

Original length, 5·675 cm. Diameter $=0\cdot02427$ cm.

$\theta=42°$. $\rho=8\cdot8$.

$q=\frac{2\cdot701}{y}\times10^{12}$. Correction for stretching, $dg=5\frac{q}{l}\cdot dl$.

Temperature.	y	$q\times10^{11}$	dl.	Correction.	$q\times10^{11}$ corrected.	Remarks.
16°C.	2·70	9·48	...	...	9·48	
84	2·72	9·42	...	...	9·42	
152	2·90	8·86	...	...	8·86	
208	2·80	9·16	...	...	9·16	
273	2·85	9·00	...	...	9·00	
332	3·17	8·08	0·003	0·019	8·10	Viscous flow slight.
393	4·02	6·37	0·015	0·089	6·46	
464	5·40	4·74	0·026	0·114	4·85	Viscous flow rapid.
532	7·25	3·53	0·049	0·161	3·69	

Phosphor-bronze evidently follows the usual law as found for elementary metals—viz., the elasticity decreases slowly at first, but after reaching a temperature roughly half-way from absolute zero to the melting point the increase becomes more rapid, the rapidity decreasing again at the highest temperatures. The modulus of elasticity was found to be greater after heating.

The phosphor-bronze wires were only very slightly oxidised, only a slight colouring being visible after removal from the furnace.

Glass Fibre.

Drawn from a rod of Soda Glass.

Length, 5·575 cm. Diameter, 0·0166 cm. Mass m, 0·00308 gm.

Temperature.	y	q	Remarks.
25°C.	2·60	6·07	
88	2·65	5·96	
130	2·72	5·81	
211	2·82	5·60	
294	3·00	5·27	
337	3·05	5·18	
394	3·22	4·90	
454	3·40	4·63	Viscous flow slight.
517	4·00	3·94	Viscous flow considerable.

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* Koch and Dieterle used strips rolled out from wires.

The following measurements will give an accurate idea of the regularity of the fibre, which was only a chosen piece pulled out in the usual way from a rod. The measurements are of the diameter in scale divisions of the microscope, and are taken at four points dividing the wire into three equal parts :—

46·5, 47·0, 47·8, 47·0.

Glass was chosen as an amorphous substance, in contrast to the crystalline metals. It will be seen that the rapid decrease begins considerably nearer the melting point than in the case of the metals.

The above results are only typical ones, chosen from among many others, and the type of variation these numbers exhibit is shown in the following series of curves.

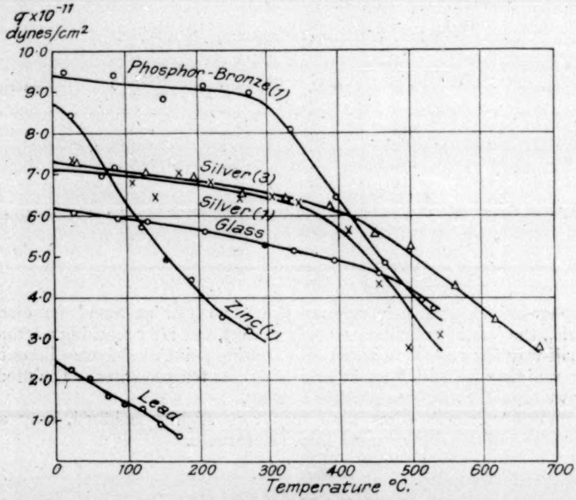

FIG. 2.

SUMMARY AND CONCLUSIONS.

With the exception of lead and zinc, the variation of Young's Modulus with temperature appears to be of the same type in all the substances tested. This variation consists of a slow decrease during the lower part of the temperature range, followed by a more rapid decrease at higher temperatures. It is probable that the apparently exceptional cases of lead and zinc follow the above rule also, but that the temperatures at which the variation is slow are below those used in these experiments.

It is found that if log q is plotted against temperature, practically all the curves consist of two straight lines. This remark applies equally well to the results of other investigators, some of which are included in the curves shown in Fig. 3. Accordingly, each curve may be represented by two equations of the type $q=q_0e^{-bt}$. The following table gives the values of the constants q_0 and b as deduced from the present experiments and those of other investigators.

If $q=\left(q_1e^{-b_1t}\right)_{t_1}^{t_2}+\left(q_2e^{-b_2t}\right)_{4}^{t_3}$, where t is Centigrade temperature.

Substance.	Observer	$q_1 \times 10^{-11}$	b_1	Range °C.	$q_2 \times 10^{-11}$	b_2	Range °C.
Aluminium ...	J.P.A.	5·7	0·00131	0–100	8·5	0·00248	200–500
,, ...	K.&D.*	6·05	0·000555	0–200	10·0	0·00262	270–500
Brass	K.&.D.	7·35	0·000511	0–400	9·8	0·00119	440–600
Duralumin ...	K.&.D.	6·0	0·000348	0–300	7·7	0·000998	360–600
Glass	J.P.A.	6·2	0·000589	0–400	...	...	...
Gold	K.&.D.	5·5	0·000163	0–330	7·45	0·00103	400–600
Iron	K.&.D.	14·3	0·000392	0–470	...	...	...
Lead	J.P.A.	...	...	...	2·5	0·00612	0–150
,, ...	K.&D.	...	...	...	2·05	0·00108	0–250
Phosphor-bronze	J.P.A.	8·70	0·000137	0–240	26·2	0·00393	300–500
Platinum ...	J.P.A.	13·4	0·0000937	0–550	...	...	...
Silver	J.P.A.	7·4	0·000437	0–380	20·3	0·00288	450–680
Steel	...	...	...	...	...	...	...
Tin	K.&D.	...	...	...	3·6	0·00153	0–210
Tungsten ...	Dodge†	35·2	0·0000751	0–900	...	...	...
Zinc	J.P.A.	...	...	...	8·6	0·00300	0–300

It may be observed that the region in which the variation changes from the first to the second value occurs roughly at the same fraction of the absolute temperature of fusion. The list below indicates this. In the list T_1 represents the estimated temperature which is midway between the end of the slow variation and the beginning of the rapid. Brass and duralumin seem to be rather exceptional.

Substance.	Absolute temp. of fusion, T_0.	T_1 (abs.).	$\frac{T_1}{T_0}$
Aluminium	930	454	0·49
Brass	1,170 (approx.)	700	0·60
Duralumin	920	580	0·63
Gold	1,330	645	0·49
Iron	1,800	760	0·42
Phosphor-bronze	1,273	573	0·45
Platinum	2,030	900	0·44
Silver	1,235	670	0·54
Silica	2,070 (approx.)	1,050	0·51
Zinc	690	275	0·46

In the cases of lead and tin no such bend was found at ordinary temperatures. If we multiply the absolute temperatures of their melting points by the mean value of the numbers in the last column (omitting brass and duralumin) we obtain for the temperature of rapid decrease 12°C. and —36°C. respectively, which sufficiently accounts for their non-appearance. Dodge investigated tungsten up to 1,000°C., but found no similar bend in the curve. From the above general rule we should expect

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* Koch and Dieterle.

† H. L. Dodge, Phys. Rev., 11, 311, Ap. (1918). The numbers are deduced from a very poor curve given by Dodge.

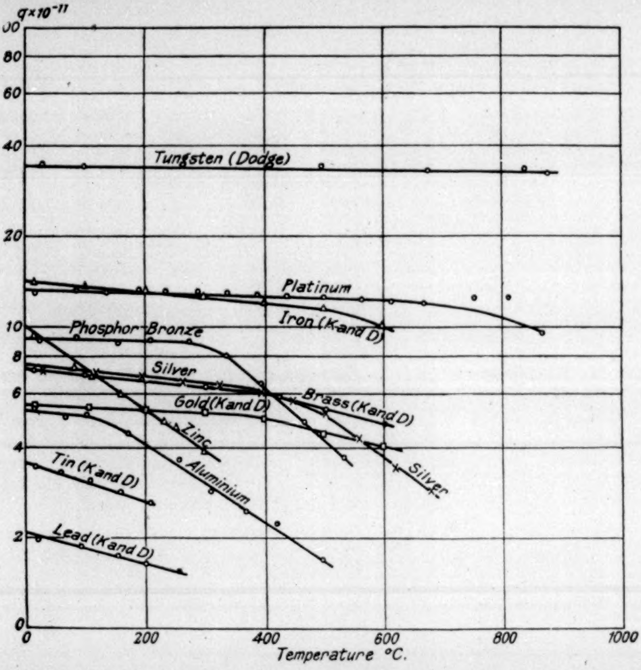

FIG. 3.

the decrease to occur at about 1,420°C., which again accounts for the apparent exception.

In conclusion, I have to thank Dr. C. H. Lees for advice during the course of this work.

DISCUSSION.

Dr. Harrison said that the formulæ employed assumed a linear relation between stress and strain, whereas measurements carried out by himself some years ago showed that no such relation holds at temperatures above the change point. The wire would then become plastic and its form would cease to be an "elastica."

Dr. D. Owen inquired whether the author had experimented with specimens of various lengths, as defects near the clamping point (where they would have a maximum effect on the results) would be checked by so doing. The value of Young's Modulus as usually found from

the deflection of a loaded bar depends on whether the bar is clamped at one or at both ends, presumably for some such reason as the above. Had the author studied the relation between his results and the change with temperature of the crystal structure of the specimens ?

Mr. F. E. SMITH pointed out that the diameter of the phosphor-bronze wire had been given in centimetres to five significant figures, and questioned the validity of the last one or two of the latter.

Capt. C. W. HUME (communicated subsequently) : May not the discrepancy in measurements on bars referred to by Dr. Owen be due to neglecting the effect of shear strain as distinct from bending strain ?

The AUTHOR, in reply to Dr. Harrison, admitted that the elastic law does not hold at high temperatures and that the term "Young's Modulus" then ceases to be appropriate. The observations were taken immediately after the application of a given stress in order to obtain definite conditions as regards the effect of plasticity, and the results so obtained represent the ratio of stress to strain under the conditions described. They may be of value to engineers who have to deal with conditions which are similar, although they are not easy to interpret theoretically. In reply to Dr. Owen : Each result given in the Paper is a fair sample of a number of observations in which various specimens were used, though the dimensions of the apparatus did not permit of varying their length within very wide limits. It had not been possible to study the crystal structure of the specimens, but single crystals had been found to have an elasticity less than the average. The numerical result quoted by Mr. Smith had been copied into the Paper in error from a manuscript calculation. The fourth significant figure could be regarded as fairly reliable.

XVI.—CRITICAL VELOCITY OF FLOW PAST OBJECTS OF AEROFOIL SECTION.

By E. G. Richardson, *B.A.*, *M.Sc.*, *Ph.D.* (University College, London).

Received February 3, 1925.

ABSTRACT.

By observations of the " Æolian Tones " of vibrators of aerofoil section, critical values for flow past an object of aerofoil section have been obtained of a fluid incident at various angles. The minimum value of VL/ν for unsteady flow is found to fall from 60 at 0° incidence to 45 at 20°, and then more rapidly.

INTRODUCTION.

THIS work is a continuation of that described in a previous Paper, entitled " Æolian Tones."* Some of the methods described therein have been applied to an examination of the " Æolian " vibration of brass and rubber models of aerofoils with a view to finding the lowest stream-velocity of fluid past the model at which a vibrator of suitable frequency, n, could be made to vibrate. It is explained in that Paper how an object of width D will vibrate in a plane at right-angles to the direction of the stream, if its natural frequency coincides with that of the eddies produced behind it, as given by the equation V/nD=constant, provided that at this velocity VD/ν exceeds a certain value, below which no eddying is produced in the wake of the object, when, in fact, the movement of the fluid is " steady." If the dimensions and tension of an aeroplane strut of such section should be such that one of its natural frequencies conforms to that of the vortices produced, it might conceivably fracture itself by the resonant oscillation set up ; cases of fracture of air-screw blades have been attributed to this cause. In the following observations the value of V for maximum resonance was determined as before when VD/ν was well above the limit ; but when the dimensions of the system were such that V lay near the critical value, the lowest speed for vibration to continue was observed, as the speed was slowly reduced, in order to get nearer to the critical velocity. Thus, in such a case, the lower extremity of the resonance curve was " observed" instead of the peak.

BRASS AEROFOILS IN LIQUID.

Corresponding values of V, n and D were found by the revolving tank method. The first aerofoil made had a diameter of 0·094 cm., and chord of 0·7 cm. It was about 20 cm. long, and was filed down from a piece of strip brass to the section shown in Figure. When this was clamped at one end, however, in the fashion used for the round rods, it was found to be too stiff to vibrate appreciably. Accordingly, a piece about 5 cm. long was sawn off and soldered at one end to a spring steel strip—an old hack-saw blade—20 cm. long. When about 3 cm. of the aerofoil dipped under water and the other end of the blade was clamped, the aerofoil was found to respond well to the vortical motion behind it, when the stream grazed the flat surface of the aerofoil.

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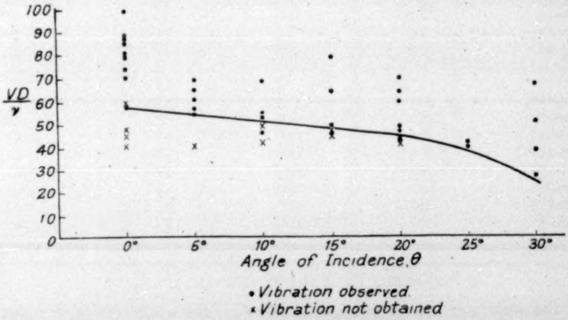
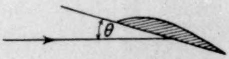
* Proc. Phys. Soc., Vol. 36, p. 153 (1924).

AEROFOILS OF ELASTIC CORD IN AIR.

In order to examine the vibration of a streamed-lined object in air, under conditions more nearly approaching actual practice, it was decided to obtain rubber bands of aerofoil section and stretch them across the frame of the " whirler " (*loc. cit.*). Mr. Niblett, the instrument maker, was able, after several failures, with great patience and care, to grind down one of the square-section cords, by means of an emerywheel turned to the required section, giving a cord, 12 cm. long and 0·2 cm. maximum thickness when unstretched.

When the cord was stretched in the whirler to several times its own length, it gave a note, when plucked laterally, of frequency between 50 and 130, as determined by the stroboscope. But the tone was too muffled to be heard distinctly when the driving motor was running. It appeared that to devise any visual method for testing whether the whirled cord was vibrating would be fraught with difficulty, but the problem solved itself as soon as the arms were rotated. When the cord was carried round the axle like a stiff rod, the appearance was that of a dull red band between the highest and lowest position of the cord, this being due to persistence of vision. When the cord was vibrating as well as moving round an axis, it became momentarily stationary, relative to the arms, at each extremity of its vibration, but moved more or less rapidly at every other phase. The extremities of each excursion of the cord were thus more marked to the eye than any intermediate position of the cord, and appeared as a series of horizontal ribs against the dark background. The incidence and cessation of the vibration were easily detected by watching for the appearance and disappearance, respectively, of these " ribs,"





and, further, as every two ribs represented one vibration, a check on the previously determined frequency could be made. In every case, it was the frequency proper to the cord under the prevailing tension.

A correction has been applied to V to allow for the drift produced by the axle and cord-holders at the small distance from the axis at which the cord was placed (about 15 cm.). It was necessary to place the cord nearer to the axis than the original wires, because the low frequencies of the former required correspondingly low velocities; too low, in fact, for the wind-tunnel to be used, where drift would not have arisen.

TABLE I.—*Some Results at Grazing Incidence.*

(A) In Liquid; $\nu = 0{\cdot}012$ or $0{\cdot}021$.

D	n	V	V/nD	VD/ν
0·094	17·0	16·2	10·15	127
0·094	11·0	10·0	9·65	79
0·094	12·2	4·5	13·5	58 (minimum)
0·20	2·7	5·5	10·2	85
0·19	5·0	12·5	13·2	100
0·19	4·3	11·0	13·3	87
0·19	4·0	9·0	11·7	80
0·39	1·0	5·0	12·7	163
		(B) In Air.		
0·065	206	375	28·0	162
0·075	120	243	28·0	121
0·085	96	130	15·9	74
0·085	108	128	13·8	72
0·085	216	235	12·8	133
0·095	120	180	15·8	113
0·115	96	283	25·8	220
0·13	86	318	28·5	276

EFFECT OF YAWING THE AEROFOIL.

As the resistance of an aerofoil varies with the angle which the flat under-surface makes with the direction of flow at a point in the fluid considerably in advance of the aerofoil, it was thought that it would be interesting to find how the critical VD/ν varied with this angle of incidence.

In order to vary this angle in the tank experiments, a little attachment was made consisting of a solid, and a hollow cone, fitting tightly together, each having a short pointer and a slot, in which were clamped the lower end of the blade and the upper end of the aerofoil, respectively; this apparatus was only 2 by 1 cm., exclusive of the pointers. The pointers were first set radially to the tank, and the blade clamped firmly to its stand above. The cone on the aerofoil was then turned 5 degrees at a time, and observations made. In this way the vibration was always constrained to be at right angles to the stream, while the angle at which the latter was presented to the "flat" of the aerofoil was given by the angle between the pointers.

Yawing on the whirler was simply effected by drifting three other sets of holes in the arms to carry the tension clamps, inclined to the original set at 10, 20 and 30 degrees respectively.

TABLE II.—*Inclined Aerofoils.*

(A) In Liquids.

Angle Degs.	D	n	V	V/nD	VD/ν
5	0·094	7·9	7·2	9·7	56·5
5	0·094	6·8	7·0	9·4	55
5	0·094	6·4	8·0	10·1	65·5
5	0·094	8·8	9·0	11·1	70·5
5	0·155	5·9	8·5	12·2	61
10	0·094	7·0	6·6	10·0	52
10	0·155	3·8	5·7	9·7	46
10	0·155	3·1	7·5	12·7	68
10	0·255	2·3	6·6	11·2	53
15	0·094	10·0	10·0	10·6	78
15	0·094	8·0	8·6	11·4	63
15	0·094	6·8	6·0	9·3	48
15	0·094	5·8	6·0	11·0	47
20	0·094	8·2	9·0	12·0	70·5
20	0·094	6·6	7·6	12·3	59·5
20	0·155	3·0	4·6	9·9	59
20	0·155	8·0	12·0	9·6	69
20	0·155	7·0	11·0	10·2	63
20	0·094	11·0	11·7	11·3	46
20 & 25	0·094	12·0	12·2	10·9	45
25	0·094	8·0	8·5	11·3	44
30	0·094	6·5	6·5	10·5	26
30	0·094	10·0	11·6	12·3	50
30	0·094	8·0	8·7	12·5	39
		(B) In	Air.		
20*	0·055	180	134	13·5	49
20 & 30	0·055	150	128	15·1	48
20 & 30	0·05	141	128	18·0	42
30	0·065	180	127	11·5	55
30	0·075	143	135	12·5	68
30	0·085	120	160	15·6	91

TORSIONAL VIBRATIONS.

Harris† has shown that a stream-lined strut, slightly inclined to the wind, such as is used in aeroplanes, undergoes violent torsional vibrations at high speeds, but does not consider that these are due to resonance with the eddies. The value of V/nD at which a tested wire vibrated most was 82. My rubber aerofoils executed similar vibrations at any angle to the wind from 0° to 45°, in a wind-channel. The oscillations were made evident by a fine needle stuck through the rubber parallel to the flat side; this no doubt encouraged the slow torsional vibrations in preference to the lateral ones. The lower limit to the velocity at which the former were observed was 300 cm. per sec. In each case, a " most favourable " speed was noticed, and the frequency was that natural to the cord under torsional oscillation. Curiously enough, the values of V/nD as obtained by the method described in the earlier Paper are nearly multiples of the number 82, obtained by Harris, pointing to some form of resonance.

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* Also at 10°, but weaker.

† Report No. 759, Aero Research Council.

*Torsional Results.**

V	580	420	420	360	350	390
n	16·5	8·0	7·1	15·8	15·3	18·5
D	0·12	0·080	0·090	0·105	0·070	0·130
$\frac{V}{nD}$	292	656	655	156	326	162
Multiples	328	656	656	164	328	164
$\frac{VD}{\nu}$	465	224	252	266	164	338

DISCUSSION OF AEROFOIL RESULTS.

The graph shows a gradual drop in the critical value as the angle of incidence is increased up to about 20 degrees, and this is there followed by a sharper drop. A corresponding rise in the resistance or " drag " in the region of this angle is usually observed in an aerofoil of this type, but the slight fall of resistance between 0° and 5°, which is characteristic of an aerofoil, is not apparent, but it is quite possible that this slight rise would come within the limit of experimental error, particularly as the angle was only determined to within a degree or two. With an object of given D in a given fluid, the results show that nearly twice the velocity must be attained for stream-line to pass into turbulent motion, when the object is an aerofoil at grazing incidence, than when it is a round rod. The aerofoil continues to hold this advantage as it is turned, though to a less and less degree, until the fluid is incident at more than 20 degrees, when the circular section is better than the stream-lined section of the same D. The results in liquid and those at the slower speeds in air give an average value of 12 for V/nD. Other air results at higher speeds give values double this. This is probably to be explained as a vibration of the—comparatively—loose cord in one segment due to an eddy frequency twice as great so that the latter was the octave of the vibration of the cord. For it was noticed in the earlier wind-channel experiments that a cord had sometimes to be encouraged to vibrate in two or three segments by momentarily touching it at the appropriate nodes. Alternatively, the increase of V/nD may be concomitant with an increase of VD/ν.

It is hoped that the experiments give, though by an indirect method, some critical data for aerofoil flow—a subject which has only recently been investigated, and for which, therefore, every new method should be tried. It was Dr. N. A. V. Piercy, Lecturer in Aeronautics at East London College, who suggested this work as a corollary to the earlier investigation, and I am much indebted to him for this and other suggestions in the compiling of the present Paper.

DISCUSSION.

Prof. F. L. Hopwood congratulated the author on the interesting line of work on which he had embarked. Some years ago he had found that the æolian tones or "thrumming" of tow wires under water were diminished by "bearding" the wire with leather thongs or short strands.

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* My thanks are due to Mr. E. Tyler for assistance in obtaining these.

of rope. Would not the rubber blades employed by the author be liable to distortion into irregular shapes during vibration ?

Sir **Richard Paget** said he was interested in the production of tones by stretched rubber strips in an artificial larynx, and would be glad if the author could tell him what shape of cross-section would give a maximum of sound. The tension is an important factor in such arrangements ; if the strip be not in tension when at rest, the pitch varies rapidly with the velocity of the air stream.

Dr. W. S. **Tucker** said that the subject interested him in its bearing on the production of noise by aeroplanes. Would an aerofoil vibrate sufficiently to throw the whole of an aeroplane into vibration, and if so, would it be possible to obtain in this way frequencies of, say one or two vibrations per second which might be detected by some suitable means ? The stays of an aeroplane give high-frequency notes which are masked by the low-frequency notes of the engine, but whose existence is shown on oscillograph records. Although such notes are not noticeable on big planes, small planes can be made to scream. It is difficult to know whether lateral or torsional vibrations are responsible for this effect, but probably it is the latter.

Dr. J. S. G. **Thomas** asked how the author interpreted the linear-dimensional term D occurring in the zero-dimensional formula V/nD. At what part of the aerofoil section is D measured ?

Prof. A. O. **Rankine** congratulated the author on the results he obtained with a remarkable economy of resources. Some work bearing on the present subject was described by Bjerknes at the British Association last year, and recently at the Royal Institution. Bjerknes regards the upthrust on an aerofoil as due to the eddies, one of the two oppositely rotating series of which is supposed to be left behind by the forwardly moving aerofoil, while the other series clings to the aerofoil and circulates all round it in close contact, thus changing the pressures above and below it so as to give rise to an upward resultant. In these circumstances the low-frequency vibrations suggested by Dr. Tucker do not appear likely to occur, since æolian tones depend on the co-operation of the two series of eddies.

The **Author**, in reply, agreed that the shape of the rubber strips might be deformed when vibrating with great amplitude. Of the square, circular, and aerofoil-shaped strips which he had tried, the circular gave the strongest vibrations. If it is true that V/nD is constant up to high values of V, then n could be very high, but at aeroplane speeds it probably could not become low enough to cause resonance of the aeroplane as a whole. As regards the direction of vibration, Harris observed full-sized struts in a wind channel and found very little lateral but much torsional vibration. D is the maximum thickness of the section ; the author had hoped to apply the method to the determination of the effective diameter for various shapes of cross-section, but owing to instability in the conditions no safe conclusions on this subject could be drawn. He was much interested in the information given by Prof. Rankine. Some photographs given by Calmann-Levy show (by means of smoke) the eddies in the neighbourhood of edge-on and inclined aerofoils. Both series of eddies appear to leave the aerofoil in the ordinary way, so as to produce the æolian effect, but it must be borne in mind that such aerofoils are different from struts in shape.

XVII.—FOCUSSING METHOD OF CRYSTAL POWDER ANALYSIS BY X-RAYS.

By J. Brentano, *D.Sc.*, Lecturer in Physics, Manchester University.

Received March 28, 1924.

Abstract.

The conditions of reflection of X-rays from a thin layer of crystal powder are discussed and it is pointed out that for any given angle of reflection a surface of double curvature can be found, such that it will reflect X-rays coming from one point, to any other definite point. For an element of this surface, situated so as to be distant from the two points by lengths a and b respectively, the relation $\frac{\sin\alpha}{\sin\beta}=\frac{a}{b}$ must be satisfied where α and β are the glancing angles of incidence and of emergence of the X-rays with respect to the surface.

An arrangement for crystal analysis based on this relation is discussed and expressions are given for its resolving power. An apparatus of this type for photographic work and a hot cathode X-ray bulb of simple design are described.

A new determination of the lattice constant of nickel oxide is given.

THE use of crystals in the state of fine powder for X-ray crystal analysis not only dispenses with the necessity of having large specimens with well-developed faces, but presents the further advantage of reducing selective absorption, the effect of which has been brought into prominence by the work of Sir William Bragg and of Prof. W. L. Bragg. This point is of great importance for determining more complicated structures, when the relative intensities of the lines have to be taken into account. On the other hand, in the powder method as it was originally introduced by Debye and Scherrer and by Hull, there is a certain difficulty in allowing for general absorption, as for any angle of deflection different parts of the deflected beam have to pass through different thicknesses of the powder stick. Then also the width of the lines is of the same order as the projection of the incident beam on the film and depends not only on the angle of deflection, but also on the absorption in the powder. This again makes a comparison of intensities difficult and led to the use of very narrow beams and large cameras with exposures ranging up to 400 or 1,000 milliampere-hours when greater definition was required.

Methods in which thin layers of powder are used in conjunction with divergent beams have been developed by H. Seemann, H. Bohlin, Sir William Bragg and the writer.*

The method of Bohlin and the writer's previous arrangement, in which beams of very great angular extension are used, are efficient for making rapid qualitative surveys; they are less appropriate for accurate measurements and for comparison of intensities. Sir William Bragg showed that very good results could be obtained by substituting a flat layer of powder for a single crystal face in his spectrometric method, in which an ionization chamber is used, and E. A. Owen and G. D. Preston† used and discussed this method and demonstrated its great efficiency in a number of examinations of solid solutions. The photographic counterpart, the introduction of a flat powder layer in a De Broglie spectrograph, cannot be equally effective since a powder layer does not give the well-known spectroscopic focussing effect of the single crystal face which determines the intensity and definition obtained by the photographic method using a single face. In fact, A. Müller,* who examined photographically a series of fatty acids in the form of flat layers and obtained great intensity with good definition, was dealing with a case equivalent to that of a single crystal and not to that of an irregular powder, as the micro-crystals were regularly orientated parallel to the supporting plate. In the present Paper, of which a short account has been given in Nature, Nov. 3, 1923, it is propcsed to discuss the geometry of X-ray reflection from a powder layer, with the object of determining the conditions best adapted to the photographic method.

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* H. Seemann, Ann. d. Phys., 59, p. 455 (1919); H. Bohlin, Ann. d. Phys., 61, p. 421 (1920); Sir William Bragg, Proc. Phys. Soc., 33, p. 222 (1921); J. Brentano, Arch. Sc. Phys. et Nat., (5) 1, p. 550 (1919).

† E. A. Owen and G. D. Preston, Proc. Phys. Soc., 35, p. 101 (1923); and 36, p. 14 (1923).

Consider a beam of X-rays diverging from a point *A* falling on a block of crystal powder ; if we pick out all those particles, which are so situated as to reflect X-rays through a given angle γ to the same point *C*, these are on a surface of double curvature which belongs to the surface generated by the rotation of an arc *ABC* (Fig. 1) about the chord *AC* as axis, where *ABC* is the arc of a circle such that the chord *AC* subtends an angle $\pi-\gamma$ at any point on it. This surface, which we may call a toroid, is the geometric locus of all the powder particles which satisfy the given condition of reflection ; in general, the lattice planes from which reflection to *C* takes place will be inclined with respect to the surface of the toroid like the tiles of a roof. With the points *A* and *C* fixed, a different surface corresponds to any angle γ. The arrangements of Bohlin and of the writer referred to above, in which cylindrical strips of powder were used, represent two different ways of making these strips be tangential to the toroids which correspond to a wide range of reflecting angles. Different parts of the reflected radiation are then recorded under different conditions and this makes these arrangements less suitable for quantitative work.

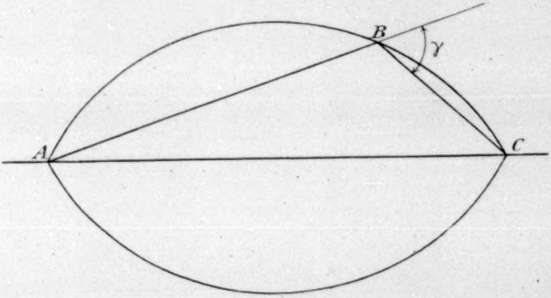

FIG. 1.

For a relatively small range of angles a spherical cap mounted on an X-ray goniometer is actually the best surface when greatest intensity with good definition is desired. For greatest accuracy such a surface presents the disadvantage common to all arrangements using reflection from one side only that parallactic errors may arise from the difficulty of locating the exact position of the surface from which reflection takes place. As will be discussed, a thin crystal powder layer offers the possibility of avoiding such errors by using the diffraction from both sides.

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* A. Müller, Journ Chem. Soc., 123, p. 2043 (1923).

If we consider a small powder area and in Fig. 2 *A* represents a slit, which is the origin of monochromatic X-rays, *B* the small flat surface of powder, and *C* the position of the recording film, and if we discuss, in the first place, only rays travelling in the plane *ABC*, then reflection will occur at any point of *B* through all those angles, which correspond to the different lattice spacings of the powder, but the rays which have been deflected at different points of *B* through a certain angle γ will fall on the same point of *C*, only when the surface *B* is tangential to the corresponding toroid surface. If we call the distance *AB* a, the distance *BC* b, and α and β the glancing angles of the surface *B* with the directions *AB* and *BC*, then this condition is expressed by the relation

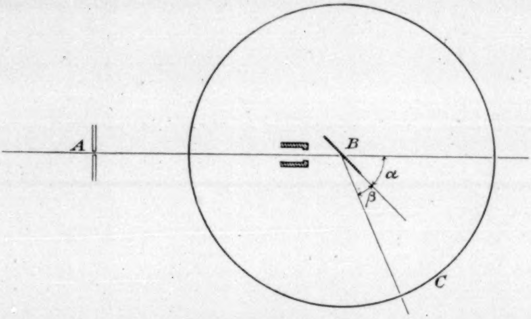
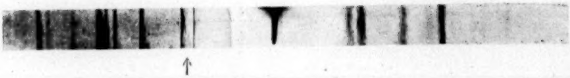


FIG. 2.

$$\frac{\sin\alpha}{\sin\beta}=\frac{a}{b}$$

since in this case a and b are chords of the same circle ; $\gamma=\alpha+\beta$ is then the angle of deflection.

Photograph I, which was taken with an arrangement of this kind, illustrates this effect. A beam of *CuK* radiation of an angular width of 5 deg. was falling on a thin layer of nickel oxide set at such an angle as to correspond to the condition of correct definition for an angle of deflection $\gamma=38$ deg. (marked by an arrow on the photograph). We see that the line near to it, which corresponds to the 222 reflection, is sharp and that the lines gradually become broader, as their distance from the position of correct definition increases. To obtain correct definition over an extended angular region, *B* has to be rotated about an axis vertical to the plane *ABC* and a screen has to be provided so that for any position of *B* only a small portion of *C* is exposed at one time.



PHOTOGRAPH I.

To face page **186.]**

If the plane B is the lattice plane of a single crystal, such a screen is not necessary. In this case, as the crystal is rotated, reflection through a given angle γ takes place from one point only at any instant, and when the distances a and b, taken from the axis of rotation, are equal, the different parts of the surface will successively reflect radiation, deviated through the same angle, to the same point on C. According to the spectroscopic focussing effect for this case of a equal to b, with a single good crystal face, an extended flat surface, lying in the axis of rotation will give correct definition. With a fine powder we have a simultaneous effect from all parts of the layer and no special focussing property is attached to the equality of the distances a and b. In its place we have the more general relation

$$\frac{\sin \alpha}{\sin \beta}=\frac{a}{b}$$

to determine the orientation of the layer for best definition. This expression, however, refers to an element of the surface only, since with a powder layer, the surface which will give correct definition for any angle of deflection is not a plane, but a toroid.

With a powder layer at B, we have therefore to consider two particular causes, which will affect definition ; one is that a flat crystal layer is only an approximation to the toroid, the other that for any position of B only one point on C corresponds to correct definition, so that if we expose a certain extension of C at one time, the lines will lose in sharpness. The first effect comes in for ionization as well as for photographic work, when wider beams are used, and has been discussed by Owen and Preston for the case a equal b. The second is peculiar to the photographic method and is not felt in ionization work, where only a narrow beam is allowed to enter the ionization chamber.

To determine this second effect, we consider the extreme widening d of a line on any point of C, which is produced by turning the layer B by a small angle δ out of the position which would give good definition for this point. This broadening will depend on the extension of the layer B from which radiation is reflected and on the angle of reflection β ; if we express the extension of the reflecting layer at B in terms of the angular width ε of the incident beam, which is of special interest, since it is related to its intensity, we easily see that

$$d=b\delta\varepsilon \cot \beta.$$

If now, instead of a displacement δ of the powder layer, we consider the displacement of the point of correct definition on the film which corresponds to δ, then if a region of angular extension η is simultaneously exposed, the point giving correct definition falling on the middle of η, we get, for a not very different from b, for the greater part of the angular range, with sufficient approximation

$$\delta=\frac{1}{2}\frac{a}{a+b}\eta$$

and hence

$$d=\frac{ab}{2(a+b)}\delta\eta \cot \beta.$$

Instead of the linear widening d of the lines, it is again useful to take the angular width ω_1, subtended at the axis of rotation of B, since the ratio of this angular width to the angle of deflection γ is an indication of the resolving power to be expected.

We obtain thus

$$\omega_1=\frac{a}{2(a+b)}\varepsilon\eta\cot\beta,$$

which gives the extreme angular broadening of the lines produced, when a region of angular extension η of the film C is exposed simultaneously.

By following a similar reasoning, the extreme limits of the broadening ω_2, due to substituting a flat powder layer for the toroid, can be expressed in angular measure by

$$\omega_2=\frac{\varepsilon^2}{4}\cot\beta.$$

The expressions for ω_1 and ω_2 show that the effect they produce on the angular width of the lines and thus the accuracy of the measurements depends only on the angles ε and η, and is independent of the linear dimensions of the apparatus. On the other hand, some other factors which affect definition, such as the thickness of the layer and in particular the scattering in the photographic film, produce a constant widening, which for given values ε and η, i.e., for equal intensities, will have a smaller relative effect with a larger apparatus.

As has been pointed out above, for making exact measurements of the angles of deflection, a thin flat layer of powder presents the special advantage that when fixed on a suitable support, it can be exposed from both sides ; by taking symmetrical exposures to the right and to the left and having the X-rays reflected from the two sides of the layer, parallactic errors due to imperfect centring can be eliminated. The angular width ε must then be fixed according to the resolving power required. When only relative measurements in a limited angular region have to be made and when using thick microcrystalline plates, a cylindrical or spherical surface can be found, which approximates much more closely to the toroid. In this case much wider beams can be expected to give good definition, particularly when a small value of η and ω_1 is given by the experimental conditions, as when working with the ionization method the very faintest reflections have to be traced.

The sharpness of the lines will also be affected by the obliquity of the rays and by the depth from which reflection takes place in those cases where the layer cannot be made sufficiently thin. The effects are similar to those which occur with the X-ray spectrometer using a single face, and have been fully discussed in connexion with this instrument.

From the relations given for ω_1 and ω_2 with a flat powder layer, it can be seen that in those cases where the angles of deflection γ are not too great, it will in general be convenient to make a small by bringing the slit at A near to the powder and to increase b, the distance of the photographic film, since in this way by increasing the angle β, ω_1 and ω_2 are reduced. Except for recording individual lines at definite angles, it is also of advantage to adjust the width of a rotating slit in front of the photographic film, which limits the opening η_1 to such a value, to make ω_1 about equal to ω_2. It will be seen that in this case the two broadening effects compensate with great approximation for one-half of the powder layer, they are additive for the other half; in this way lines showing a sharp maximum of intensity at the correct position are obtained.

For exploring more extended angular regions *a* must be larger than *b*, and the apparatus has to be so designed that the rotating screen, which will only uncover a small portion of the photographic film, at one time, does not intercept the incident X-ray beam.

Fig. 3 gives a diagrammatic view of the apparatus. In *A* is a fine slit on the side of the incoming X-ray beam, *B* is a brass frame, which supports the thin celluloid film coated with crystal powder, which can be centred with respect to the axis *E*. *D* is a graduated disc, from which the orientation of the powder layer can be read.

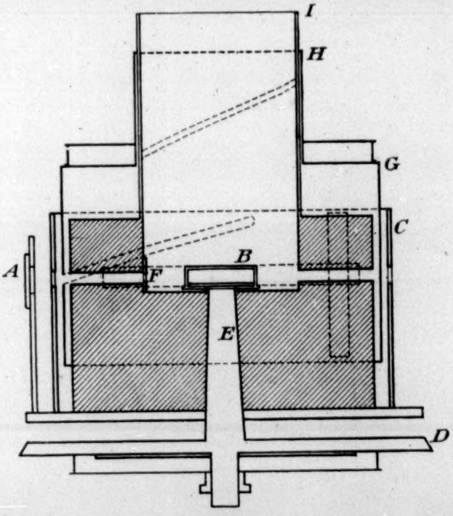


Fig. 3.

The axis can be rotated by pressing a pulley against *D*. In *F* are screens to limit the divergence of the incident beam. The film carrier consists of a brass cylinder *C*, 10 cm. in diameter, the outside of which is carefully turned concentric to the axis *E*. This was assured by actually rotating the apparatus about the axis *E*, holding the axis fixed in the lathe. The photographic film is pressed against this cylinder from outside and the deflected X-rays reach it through horizontal slots cut in *C* at the height of the powder layer. These slots extend on both sides so that exposures in an angular range of γ from 12 deg. to 150 deg. can be taken; they are covered on the inside with blackened celluloid to prevent light exposure. The film carrier can be removed from the apparatus. *G* is the rotating screen, which moves just inside the film carrier. It is held by a brass tube *H*, which screws on a steep thread on the cylindrical piece *I*, so that when the screen is rotated an axial displacement takes place in the same time. Fig. 4 gives a view of this screen. *K* is an adjustable vertical slot, the width of which determines the portion of the film to be exposed at one time. *L* is a narrow helical slit, with the same pitch as the thread on *H*. When this slit is made to correspond to the incident beam of X-rays, it will allow free entrance of the beam for any position of the screen, while the photographic film is protected except for the part facing slot *K*, which in succession uncovers different portions of the film, as the screen is rotated. Screen *G* is also provided with a graduated dial and with a pulley, and both the powder layer and the screen, can be rocked within the angular region required, by means of thin steel wires

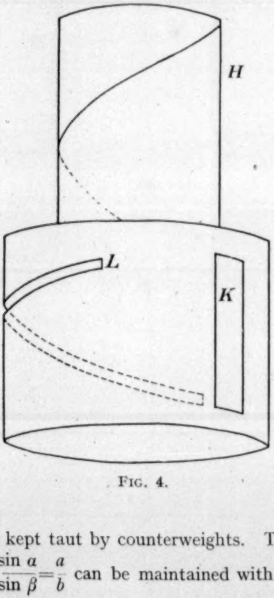
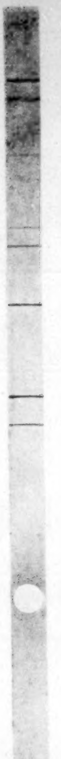


FIG. 4.

fixed at the pulleys and kept taut by counterweights. The correct rate of motion to satisfy the condition $\frac{\sin \alpha}{\sin \beta}=\frac{a}{b}$ can be maintained with sufficient approximation, by moving the wires from discs, disposed excentrically on a common axle. To record the reflection from a single crystal face the same arrangement can be used; *a* and *b* have then to be equal and the rotating screen reduces the effect of general scattering.

When using beams of great angular divergence it is of advantage to bring the slit as near as possible to the anticathode. A hot cathode bulb has, therefore, been designed, in which the beam diverging from a point inside the bulb could be used.

Hot cathode bulbs have been described by Siegbahn, Kirchner, Shearer, Deauvillier and others. The present bulb, which proved efficient for the purpose required,



PHOTOGRAPH II.

[*To face page* 191.

has been made so as to give the greatest simplicity of manipulation. The cathode, which is kept at high potential, has been so designed as to need no cooling; the possibility of a gas discharge taking place with imperfect vacuum has been reduced by avoiding large open distances between the metal parts of cathode and anode; only one sealing wax joint is used.

The glass part *A* in Fig. 5 carries on one side a lamp socket for the connection to the heating battery, which is insulated and connected to the negative side of the high potential supply; on the other side it supports a quartz tube *B*, at the end of which the filament is fixed, surrounded by a short metal sleeve *C*. In this way the heat developed by the filament is dissipated by radiation to the central part *D* in gun metal, which is earthed and water-cooled, without appreciably heating *A*. Part *A* is inserted by a ground joint into the glass tube *E* sealed to *D*. From *D* a tube goes to the pump, this tube also serves to hold the bulb in position. The water-cooled anticathode *F* is fitted by another ground joint into *D*. Both cathode and anticathode can easily be removed, and it was found that the bulb is ready to work a few minutes after having been opened. The X-rays leave the anticathode through a window cut in *D*, and covered in the case of *CuK* radiation, with a nickel foil 0·01 mm. thick, soldered on to it. This nickel filter greatly reduces the β and γ components by selective absorption, and leaves radiation which practically consists of $K\alpha$ and $K\alpha'$ only.

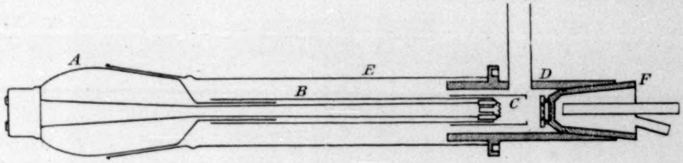


FIG. 5.

Fig. 6 gives a view of the arrangement of the anticathode adopted instead of the outside slit. *G* is a disc, with a small central opening, which prevents stray cathode rays from reaching the anticathode. In the latter grooves are cut, so that only X-rays produced on the small strip *H* can reach the window. The projection of the flat surface of this ridge in the direction of the X-ray beam represents the width of the slit used. For beams with smaller angular divergence, a flat anticathode and a slit outside the bulb were used.

The area of the anticathode spot depends on the distance between the sleeve and the anticathode; with a distance of 4-5 mm. sharp focussing is obtained and the anticathode will then be punctured with a current of about 9 milliamperes, which represents the limiting current, at which the bulb can be run under these conditions.

In order to use the full electronic output and avoid undue heating of the filament high-tension current of 500 cycles rectified by a rotating spark rectifier was used in conjunction with a condenser. For *Cu* radiation the voltage was 31,000 to 33,000 volts; it was measured by an electrostatic voltmeter.

Photograph II shows an exposure taken with *Cu Kα* radiation of nickel oxide powder, the structure of which has been found by W. P. Davey and E. O. Hoffman* to be cubic of rock salt type. The angular range recorded extends from 35 deg. to 113·5 deg. The divergence ε of the incident beam was 4·2 deg., the angular width of η was 7·1 deg., the width of the slit in A was 0·07 mm. The exposure was about 62 milliampere-hours for the right-hand side and 20 milliampere-hours for the left-hand side, in order to reduce the broadening of the stronger lines due to the scattering in the photographic film.

On the right-hand side, for the larger angles of deflection, doublets can be seen, corresponding to the lines $K\alpha$ and $K\alpha'$, which are separated by 0·00386 Å. With $K\alpha$ equal 1·5373 Å, this corresponds to a relative distance of the lines of 0·0025 of the angle of deflection. In a Zeiss comparator the position of the centres of these lines could be determined on the original film to one-tenth of their distance, or to 0·025 per cent. of the angle of deflection. This value, expressing the accuracy, with which the relative distances of lines in the region of larger angles can be measured, will obviously not apply when measurements of these angles have to be derived from the distances of corresponding lines on the two sides of the film, since the possibility of a small alteration in the length of the film during developing and fixing must be considered.

Owing to the simple and even distribution of lines of the face centred cubic lattice, and to its great density, nickel oxide is particularly suitable as a standardizing substance for crystal powder determinations with $Cu\ K\alpha$ radiation ; by mixing some nickel oxide to the powder under examination, the nickel oxide lines can be used as points of reference. Owing to its density this can be done with only a small part of the volume being taken by nickel oxide.

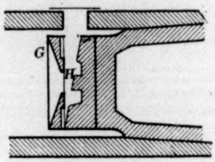


FIG. 6.

For this purpose and in connection with the work which is in progress, a redetermination of the lattice constant of nickel oxide has been made by comparing it with rock salt. Taking the value for the 100 spacing of rock salt 5·628 Å, the corresponding value for nickel oxide was found 4·172+0·004, which falls between the determination of Davey and Hoffman giving 4·20, and the later determination by Davey† giving 4·14.

The times of exposure will depend on the angular extension of the region explored. With a certain width of the slit A and with certain values of ε and η corresponding to a given resolving power, it is useful to refer to the exposure which each individual part of the photographic film receives ; the exposure for any angular range will then be obtained by multiplying this value with the range recorded and dividing by η. In these terms the average exposure in the case of nickel oxide was 5·5 milliampere-hours for the right, and 1·75 milliampere-hours for the left side.

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* W. P. Davey and E. O. Hoffman, Phys. Rev., 15, 333 (1920).

† W. P. Davey, Phys. Rev., 17, p. 402 (1921).

The method is at its greatest efficiency for exploring small angular regions, in taking exact measurements of a few characteristic key lines, but the relations given show that even for taking more extended surveys, the gain of intensity due to the angular width of the incident beam, counterbalances the effect of the limitation of the angular region which can be explored at one time.

In conclusion, I wish to thank Prof. W. L. Bragg, F.R.S., for the facilities put at my disposal, in particular for the use of the high-tension equipment obtained by means of a grant from the Royal Society. The apparatus was made by Mr. W. Wilson, London, to whom I feel obliged for the great care taken in constructing it.

DEMONSTRATION OF A KINEMATOGRAPHIC STUDY OF THE FORMATION OF PLATEAU'S SPHERULE.

By C. R. Darling, *F.I.C.*

SOME years ago Mr. Darling showed the formation of a drop at a rate which could be readily followed by the eye, except as regards the moment when the drop is breaking away from the rest of the liquid from which it was formed. This stage of the process, which takes place too rapidly to be seen in detail, has now been photographed, and a section of the film was projected on the screen. In the original experiment, which was repeated, use was made of orthotoluidine, a dark red liquid which has the same density as water at about 23°C., but becomes slightly heavier than water at lower temperatures. This liquid is allowed to flow slowly into the bulb of an inverted thistle funnel which is supported mouth downwards in water ; when a sufficient body of the liquid has collected, a large drop is formed which gradually changes shape until it breaks away, leaving behind a small secondary droplet known as " Plateau's spherule." Kinematographic study of the breaking process shows that the main drop remains approximately spherical, the liquid from which it is suspended extending downwards in a sharply pointed cone attached to the main drop at its point. After breaking away, the main drop falls, oscillating considerably in shape. The rest of the liquid snaps back to form a more or less spherical surface, but the point of the cone is left behind to form the droplet referred to ; this is given a slight upward impulse before it also falls to the bottom of the water.

Mr. Darling also exhibited a remarkable meteorological photograph taken by lightning flash by the late Mr. Leadbetter during a cloud-burst. The objects visible in the photograph are believed to be columns of water corresponding to the conical point of the cone referred to above. The taking of the photograph coincided with a heavy cloud-burst producing a flood two feet deep.

XVIII.—ELECTRICAL CHARGES ARISING AT A LIQUID-GAS INTERFACE.

By H. W. Gilbert, *B.Sc.*, and P. E. Shaw, *M.A.*, *D.Sc.*,
University College, Nottingham.

Received January 26, 1925.

Abstract.

The various methods of studying the potential differences and electric charges which arise at the liquid-gas interface are reviewed and discussed under the following headings :—

I. The determination of P.D. at the interface of liquid-gas when one or both of these is at rest.
II. Cataphoresis of gas bubbles.
III. Passage of gas over a liquid, without rupture of the latter, or of a gas over a wet solid with the same proviso.
IV. The fall of a liquid in an unbroken column through a gas.
V. Liquid jets.
VI. Waterfall electricity.
VII. Electrification produced by bubbling gases through liquids.
VIII. Electrification produced by shattering drops in an air stream.
IX. Electrification produced by spraying a liquid.

An attempt is made to co-ordinate the material obtained from this vast area of research. Many of the results may be explained in terms of the modern theory of orientation and polarisation at the liquid-gas interface, but there are other facts which do not appear to come within the scope of any established principles. Suggestions are made as to possible developments in future research.

Free surfaces of compact matter—whether solid or liquid—are the seat of stresses which give rise to molecular adjustment, film formation, and possibly electrical separation. As such surfaces are easily accessible, they provide a promising field for the study of the constitution of matter.

There are five types of definite interface : Solid-solid, solid-liquid, solid-gas, liquid-liquid, liquid-gas. Of these, the last has received the greatest attention experimentally, being open to investigation in a variety of ways. It is the type discussed in this Paper, and we have attempted to give a summary of existing knowledge on this subject. The methods employed may be arranged under nine headings :—

I. The determination of potential difference at the interface of liquid-gas, when one or both of these is at rest.
II. Cataphoresis. The movement of a gas bubble through a liquid, by the action of an electrical field.
III. The passage of a gas over a liquid, without rupture of the latter ; or by a gas over a wet solid, with the same proviso.
IV. The fall of a liquid in an unbroken column through a gas.
V. Jets. A liquid column issuing from an orifice into a gas and breaking under the action of surface tension.
VI. Waterfall electricity. Liquid columns falling through a gas on a solid or liquid surface, after breaking up under surface tension.

VII. Bubbling. The rising of a gas bubble through a liquid with emergence at its surface.

VIII. Drop-splitting. The shattering of drops by the impact of a flowing gas.

IX. Spraying of liquid by a gas, as in the action of a scent-sprayer.

Of the above sections we notice that the first is the only statical or quasi-statical method. It has been little exploited so far, but deserves attention, for, like surface tension, it yields definite information as to surface energy. The second section is of peculiar theoretical importance. The remaining sections form a large group-subject concerned with the clash of liquid with either liquid, solid or gas, under varying conditions.

We make no attempt to describe the apparatus employed in the above methods, the processes involved being in general of a simple nature, but restrict ourselves to collating experimental results and thence deducing certain general principles.

It is impossible in a summary like this to do justice to the care and skill displayed by the many investigators who have contributed to our present knowledge. Mention will be made only of some of the more prominent, or recent, contributions to the subject. The bibliography appended in connection with each section will, however, show what authorities have been consulted.

Christiansen's term *balloelectricity* will be used to apply to electric charges, of either sign, arising from the tearing of liquid surfaces in the presence of gas.

For brevity we employ the symbols (+) and (—) in place of "positive" and "negative" respectively.

The sections classified above will now be taken in order.

I. The Potential Difference on an Interface at Rest.

Kenrick$^{(1)}$, using a method invented by Bichat and Blondlot, measured with an electrometer the P.D. (V_1) between the ends of a train of five materials as follows: A normal KCl solution, a liquid A, air, a liquid B, a normal KCl solution. Next he reduced the train to four members by eliminating the air, and again measured the P.D. (V_2). The difference $V_1 - V_2$ shows the P.D. from liquid A to liquid B through air. This is clearly a differential P.D. determination of the two liquid-air interfaces.

Kenrick's results, obtained by change of materials at one of the interfaces, is as follows:—

(1) In general, a P.D. exists at a gas-liquid interface.

(2) The velocity of building the P.D. at the interface is, in general, very great. With some organic substances, however, this velocity is slow.

(3) The P.D. at the interface between solutions of highly dissociated electrolytes and air is apparently small.

(4) Certain organic substances added to a solution such as KCl in water give rise to a great decrease in P.D. at the interface. It is found that the more the lowering of surface tension the greater is the decrease in P.D. produced.

(5) The P.D. is only slightly dependent on the nature of the gas in contact with the liquid.

It is clear from these results that at a gas-liquid interface the liquid is the all-important member in the production of P.D., and that its power in this respect is influenced by the nature of any materials it may have in solution. The explanation of the contrasting results (3) and (4) is that the organic materials mentioned are positively, and the electrolytes negatively, adsorbed.

Professor F. G. Donnan[2] quotes results by T. Thorwaldsen, who has extended this method. If a substance such as hydrochloride of methyl violet be introduced into water it reduces, or possibly reverses, the P.D. This action is attributed to the adsorption of the basic dye cation to the water-air interface.

Frumkin[3] has quite recently described a new method. At a point a few millimeters over a surface of water is placed a small burning jet, in which is inserted a platinum wire joined to an electrometer. The water is earthed through a normal electrode. Thus the P.D. water-air is found as in Kelvin's method for finding P.D. When a monomolecular layer of oil, such as olive oil, is placed on the water this P.D. decreases. The same author[4] also used Kenrick's method. He dissolved various alcohols, fatty acids, and esters in normal KCl solution, and used this as liquid A, whilst liquid B (KCl) remained constant. Varying the concentration he found the P.D. values for these organic solutes. Each family has its own P.D. extrapolated for infinite concentration (e.g., acids +0·35 volt ; esters +0·61 volt). The results accord with the Gibbs's adsorption formula $q=-\frac{c}{RT}\cdot\frac{d\sigma}{dc}$. They also indicate that orientation of adsorbed organic molecules occurs, as in the experiments of Hardy, Langmuir and Harkins, though in the latter cases the organic liquid is placed on the surface, and is not usually completely soluble.

II. CATAPHORESIS OF A GAS BUBBLE.

Here measurement is made of the velocity of a small gas bubble impelled through a liquid by a constant potential difference maintained between two parallel electrodes in the liquid.

The modern researches on this subject are due to Coehn and Mozer[5], McLaughlin[6], McTaggart[7, 8, 9], and Alty[10]. We confine ourselves to the results obtained by the last two observers.

McTaggart showed that (*a*) the velocity of a bubble in water varies as the applied field ; (*b*) the velocity is the same for bubbles of air, oxygen and hydrogen ; (*c*) in water the movement is towards the anode ; (*d*) in non-electrolytes such as pure alcohol there is no movement of the bubble. In his third Paper, quoted above, McTaggart works with bubbles in very dilute $Th(NO_3)_4$, a salt which is an active reversing agent in cataphoresis. Having freed the solution of dissolved gas, in vacuo, a bubble when introduced into the liquid dissolves and shrinks slowly enough for measurement of mobility for any desired size of bubble, from 0·62 mm. to 0·05 mm. diameter, to be made. In each experiment the bubble charge is first (−), then reduces to zero as the bubble dissolves, and later becomes (+), with reversal of motion in the field. This reversing action is proved to be due to the formation of colloidal $Th\,(OH)_4$ formed by hydrolysis of $Th\,(NO_3)_4$, and the opinion is expressed that this hydrate gathers *H* ions from the water and carries them to the surface, where they replace the OH ions normally existent there. The reversal point is in all cases attained when the bubble has dissolved to much less than its initial size.

Alty's quite recent work, while confirming much of the above, has established further valuable results, some of which are: (*a*) The velocity of the air bubble in water is a complex function of its diameter; the maximum velocity per unit gradient is 4×10^{-4} cm./sec./volt/cm., for a bubble of diameter 0·1 mm. (*b*) A bubble does not acquire its full charge, and so does not attain its full velocity, immediately it enters the liquid. (*c*) If the gas is soluble in the liquid, the bubble shrinks as it moves, and the rate of gas absorption greatly affects the velocity. But (*d*) if the liquid be saturated with gas there is a negative absorption, and the bubble grows for a short time after entering the liquid. (*e*) In very pure water (i.e., of conductivity less than $2{\cdot}5 \times 10^{-6}$ ohms^{-1}) the bubbles scarcely move, whereas in water of conductivity $8{\cdot}5 \times 10^{-6}$ ohms^{-1} the velocity attains a maximum. (*f*) Applying his own data for velocity to Stokes's well-known viscosity formula, Alty calculates the number of ions per sq. cm. of bubble when this attains to maximum speed.

Alty's first result, as above, is that the velocity of the bubble is a function of the diameter, whereas, as he points out, previous theory indicates that velocity would be independent of diameter. The well-known expression deduced by the Helmholtz-Lamb theory is

$$v = \frac{V \, . \, K \, . \, X}{4\pi n}$$

in which the velocity v is independent of the diameter. That the theory due to the latter writers needs modification has been pointed out for several years by Porter(11), Freundlich(12), Mukherjee(13), McBain(14), and others. The above experimental results add further evidence on this point. The double electric layer, according to the modern view, does not consist of two closely fitting skins, (−) and (+) round the bubble, but of a skin next the bubble, say (−), and a diffused cloud, say (+), outside it. Most of the (−) ions in the inner skin are "covered" by (+) ions, but there is at any instant a residuum of (−) not covered, which constitutes the (−) charge to which the bubble owes its motion in the field. Alty interprets the third of his results thus: When gas molecules are absorbed into the liquid from the bubble they move through the (−) skin, and, entering the (+) zone, upset the equilibrium there, displacing (+) ions, and so the net (−) charge increases and its velocity rises.

Some of the results of McTaggart and Alty are deducible from the Helmholtz-Lamb theory modified in respect to the composition and extent of the double electric layer. Others, such as Alty's second and third points, are explained by Alty in terms of modern views of ionic adsorption and of film formation; but one outstanding result still calls for explanation—viz., the complex relation of velocity of bubble to its diameter.

As we shall have occasion later to refer to the interfacial P.D., it may be well to point out here that there are two kinds of potential difference. Freundlich has shown that the P.D. acting in cataphoresis is not so deep seated and not so great as that measured in Section I for surfaces at rest. The former he terms the electrokinetic, and the latter the thermodynamic, P.D.

In cataphoresis we have a phenomenon easy to study, since the controlling agent is an electric field which is readily adjusted and measured. In balloelectricity (Sections IV to IX), on the other hand, as we shall see, we are confronted with complex dynamical conditions arising in the change of shape or area of liquid surfaces. It is hardly surprising, therefore, that the theory of balloelectricity is far less advanced than that of cataphoresis. We will attempt to construct a model of the air-liquid interface, looking to cataphoresis for much of our definite information. Imagine a liquid-air surface to be suddenly formed by the introduction of a gas bubble into a liquid composed of bipolar molecules and possibly of polymeric groups of these. We suppose changes occur somewhat in the following order :—

(*a*) Freed electrons, if any, caught on the " raw " liquid surface thus suddenly produced, tend to move into the body of the liquid, owing to their mutual repulsion.

(*b*) The surface monomolecular layer of liquid now becomes oriented. In water, at least, the inner layer (that next the bubble) is (—), the outer is (+), and Alty, in the paper quoted, gives evidence that the outer layers exert a repelling influence on the free (+) ions in the body of the liquid until its charge is neutralised by the adsorption of (—) ions. See also the work of Hardy(16), Langmuir(17) and Adam(18) on orientation. With regard to this postulated orientation, Edser(15) goes further, and suggests that the whole liquid, and not the surface layer only, becomes oriented in time. The surface orientation is a quick process and is followed by

(*c*) A slower adsorption of gas molecules from the bubble to the exposed inner side of the oriented layer. This adsorbed gas forms a layer several molecules thick inside the liquid, the strength of attachment of the several layers decreasing with distance from the liquid surface. This conception of an adsorbed gas layer many molecules thick is based on the work of many recent investigations, for instance, Evans and George(19) and Manley(20) on the solid-gas interface, and Iredale(21) on the liquid/gas interface. A gas film is not as firmly attached to a liquid as to a solid surface ; for, among other disturbances it is subjected to bombardment by liquid molecules as they emerge from, and return to, the liquid surface. As Edser points out (loc. cit.) a liquid surface is, in general, much disturbed in this way. Molecules from the adsorbed gas film will be absorbed into the liquid, others taking their place from the gas in the bubble ; this process continuing (rapidly if the gas be very soluble) until all the gas in the bubble has gone into solution.

If the liquid be a non-electrolyte, free ions do not exist, and the process about to be described, which is a necessary precedent of cataphoresis, does not take place.

(*d*) If, however, the liquid contain electrolytes, (—) ions move through it to the interface and are adsorbed there by what some writers term " chemical " adsorption. The first effect of this adsorption of (—) ions is, as we have already mentioned, to neutralise the resultant (+) field of the oriented layer. When this neutralisation has been attained, the net (—) charge on the bubble (and consequently the speed of the bubble) reaches a maximum. But as further (—) ions are adsorbed they create a field which brings up (+) ions, and the double electric layer formed by these (—) and (+) ions grows until it is complete. This layer attains a state of *dynamical* equilibrium, since both its (—) and (+) components incessantly lose and gain ions by diffusion, by heat motions of liquid molecules, and by the passage from the bubble of dissolved gas molecules.

When, as in cataphoresis, an electric field is applied to the bubble the (—) skin of the double electric layer is impelled up stream, but being strongly attached to (1) its covering (+) ions on one side and to (2) the oriented layer on the other, it carries with it these and (3) the adsorbed gas films as well as (4) the gas bubble itself, in its migration to the anode. Of the (+) cloud of ions, the small residuum which does not act as cover to the (−) skin, moves down stream to the cathode.

If the liquid in use be water of conductivity of the order 5×10^{-6} ohms^{-1}, the water ions are OH (−) and H (+). When to this water is added a salt, the ions of the salt are adsorbed to the interface, bringing about changes in the films and affecting the mobility of the bubble. In the case of some salts the mobility is merely reduced; where, as in the case of those possessing multivalent cations, such as $Th(NO_3)_4$ and $La(NO_3)_3$, the mobility is reversed, showing that the net charge on the inner skin of the bubble is(+), the original (−)OH ions having been replaced in part at least by the (+) cations.

III. PASSAGE OF GAS OVER LIQUID.

A succession of writers, Allessandri(22), Wolf(23), Bloch(24), Rey(25) and J. J. Nolan and Gill(26), have all demonstrated that no balloelectricity arises by the mere rush of gas over a liquid surface, even though the latter be raised into ripples.

Again, Kelvin, Maclean and Galt(27) in 1895, and Himstedt(28) in 1902, showed that air passed over wet pumice or coke acquires increased conductivity; but as in neither investigation is it made clear that the ionization thus found in the air is not in reality due to the breaking of the liquid attached to the solid surface in a species of bubbling, we must exclude the results for the purposes of this section.

Several investigations have been made in connexion with evaporation and boiling. Faraday(29) established that simple evaporation of water, as also condensation of water vapour, is unproductive of charge. Again, Blake(30), Schwalbe(31), Beggerow(32), Gallarotti(33), and Henderson(34) have shown that evaporation of water, charged or uncharged, does not give rise to charges. Evaporation and condensation are processes which involve the passage through a surface of *single* molecules. Even in the violent process of the boiling of water the emerging vapour appears to heal any ruptures in the surface which it may produce in breaking through, and so precludes any escape of electrons or ions. So, as in none of these cases is the liquid surface torn into a "raw" condition, no balloelectric charges arise. Holmgren(35) claimed to have found (+) charge in water and (−) on the air when the latter produced ripples on the surface; but J. J. Nolan(26) and Rey(25) have recently disputed this result.

IV. FALL OF A LIQUID COLUMN THROUGH A GAS.

Both Lenard(40) and Aganin(36) have shown that if a jet of liquid meet a liquid surface *before breaking up* under the action of surface tension, no charge is developed in it or in the gas through which it passes. We note that the criterion for charging is the rupture of a liquid surface.

We learn from this and the preceding section that mere *friction* between gas and liquid does not, *per se*, cause electrical separation. Later we shall see that any process of rapid *rupture* of a liquid surface does, in general, generate charges.

V. JETS.

Lord Rayleigh(37) published a series of researches on the conditions, dynamical and electrical, in a jet of liquid rising nearly vertically from an orifice. The column of liquid breaks under surface tension into a swarm of droplets varying greatly in size and speed. Consider three cases : (1) Just after the breaking point of the column there is a region in which the droplets collide and rebound, and we have the familiar scattering of the drops. (2) When a small electric charge is brought near the jet, a potential difference is established within the jet. Rayleigh showed that if this potential difference lies between certain limits the drops acquire differential electrification, and in consequence are attracted to one another so strongly that if they collide in the jet their skins break and coalescence occurs. (3) If, however, a greater potential difference be established by bringing the electric charge nearer, the large mutual repulsion of the similarly charged drops prevents collision, and scattering once more occurs. Thus we have scattering due in the first case to collision and rebound, in the second to electric repulsion.

In connexion with the phenomenon of collision (1 above), Rayleigh showed that there exists a large electrical insulation between two clean water jets if they meet before breaking up. We have already noted that a water surface has a monomolecular oriented layer, and possibly the beginning of an adsorbed gas layer, provided a small fraction of a second has elapsed since the time of formation of that surface. These films are in composition unlike the body of the liquid, and we suggest may provide both the strength necessary to resist coalescence and this high insulation which is not broken down even by the considerable shock of collision. Hiss(38) showed that 0·008 sec. after the formation of a water surface the surface tension has fallen 9 per cent. from its first value. This result indicates that film formation is a rapid process.

Another observation made by Rayleigh was that a small amount of dust or grease on the jets, by upsetting the continuity of the surfaces or by lowering the surface tension, conduces to coalescence. Again, certain gases, especially soluble ones—e.g., carbon-dioxide and sulphur dioxide—surrounding the jet have a like effect.

Burton and Wiegand(39) in experiments on jets, applied known electrical fields near the orifice, and found (1) that the size of the drops is not affected by the field ; (2) that however large the applied field there is an absolute alignment of the drops for at least a part of their journey ; (3) for small fields coalescence occurs in the region of low velocity—i.e., at the summit of the trajectory. These authors suggest a theoretical treatment of the problem differing from that of Rayleigh. To account for the action of droplets on one another, they employ Kelvin's solution of the problem of the interaction of spheres charged alike.

Aganin(36) employed very small orifices to produce jets. He observed the jets with a microscope at a point just beyond the breaking point, and found that at this point the speed differences of the droplets are so great that collision is frequently followed by coalescence. Further along the jet, speed differences are reduced by viscosity of the air, and hence, as already observed by Rayleigh, coalescence no longer results from collision.

Two other points were established by Aganin.

(1) No charge ever arises at the orifice itself.

(2) In the earlier history of the broken jet, when coalescence of drops occurs, the air surrounding the jet receives a charge.

We shall see in later sections that the *rapid* tearing of a matured water surface generates charges. So, in the breaking up of a jet by surface tension we might expect like effects, the surface in this case also being torn after maturing. But, among balloelectric processes, jets are peculiar in that, in their case, the breaking of the surface is spontaneous. Except in the last stages of the pinching off of the drops, the formation of new water surface is a relatively slow process. In the limit, when the drop is actually pinched off at the breaking point of the jet, relatively few molecules remain in the cross section of the "neck" between it and the column; and the actual tearing process is confined to these few. As each pair of molecules torn asunder would liberate only one or two ions, the total charge arising would be small. Lenard(54) in his Paper on the general theory, allows that *small* charges may arise in this way; while J. J. Nolan and Enright (60, p. 3) state definitely that a small charge does arise.

Kelvin, Maclean and Galt(27) surrounded a column of water falling from a tap by a long tube joined to an electrometer, and observed charges in the air. But if, as seems possible, any falling drops struck the tube, and so generated waterfall electricity, their result must be omitted for the purposes of this section.

VI. WATERFALL ELECTRICITY.

This is Lenards name for a subject of which he is the pioneer. Here we deal with the conditions arising when columns of liquid in passing through a gas break under surface tension into drops and then strike solid or liquid surfaces. Lenard and Aganin have shown that no charges arise unless the column breaks before striking occurs.

The influence of the gas through which the liquid (water) passes was investigated by Lenard(40) and by Sir J. J. Thomson(41) with proportional charges as below:—

	Thomson.	Lenard.
Air	+1·0	+1·0
Coalgas	+0·41	+0·86
Hydrogen	—0·11	+0·65

These figures show the relative charges developed in the water after impact. The results differ greatly in amount, and in one case in sign, and we have an instance of the great influence exerted in this class of experiment by small—scarcely detectable—differences in the composition of the water or of the gases used.

Lenard allowed various liquids to descend through air, the results for the charges found in the liquid being, e.g., water (+1·0), ether (+0·01), mercury (+14·3).

In the above experiments the charges in the *liquid* are measured. Other investigators, Becker(42), Aselmann(43), Kähler(44) have measured the charges in the air over the liquid. For pure water Aselmann and Kähler found only (—) ions; but for solutions of common salt, both (+) and (—) ions in the air. Becker experimented with mercury falling through various gases (air, carbon dioxide, hydrogen) on to the surface of various metals. The resulting ions in the air are in some cases (+), in others (—), the numerical values also differing very greatly according to the metal employed. He concluded that each metal has its own peculiar double electric layer in air, and that the resulting charges are brought about by the joint action of the double electric layers of the mercury and of the metal struck. Becker drew up a balloelectric series, analogous to the electrochemical series, as follows:—

+Zn, Al, H_2, Hg, Air, CO_2, Fe, Cu, Pt—

The interpretation of this series is as follows :—

If mercury fall on mercury in hydrogen, the gas becomes (+), but in air or carbon dioxide (—). If, however, mercury falls through carbon dioxide and descends on iron the gas is (—) ; if on aluminium (+).

Lenard(40), J. J. Thomson(41) and Rey(25) have shown that an electrolyte such as sodium chloride, when in small concentration in water, reduces the (—) charge in the air, and when more concentrated reverses the charge to (+). Rey has also shown that raising the temperature of the solution in such cases gives rise to increased air charge, unless the concentration of the salt is at the critical point required to give a balloelectrically neutral solution.

VIII. BUBBLING.

In experiments under this heading, gas bubbles rise through a liquid until they break at its surface ; and measurements are made of the charges arising either in the liquid or in the gas above.

In the case of water this process gives rise to (+) charge in the liquid, (—) charge in the gas. De Broglie(45) divides liquids into (*a*) *active*, such as water, alcohol, ether, sulphuric acid, which give rise to charges in the air bubbled through them ; (*b*) *inactive*, such as benzene, toluene, chloroform, which give no such balloelectricity by bubbling. There does not appear to be any chemical or physical basis for this classification into active and inactive, and researches later than the above have raised doubts as to whether any such sharp division of liquids into two classes is valid ; whether, after all, the balloelectric difference between liquids is not one of kind but only of degree and sign.

Bloch(46) announced the interesting result that when an active liquid covers an inactive one, or vice versa, no charges arise on bubbling. This point ought to be investigated further, as it should throw light on the difficult problem of the nature of balloelectric activity.

Kelvin, Maclean and Galt(27) found that results depend on the nature of the bubbling gas. Thus, carbon dioxide, air, hydrogen and coal gas gave respectively, per minute, 8, 4, 2, 1·4 volts potential to the water through which they bubbled. Let us contrast this with McTaggart's results (Section II) that the nature of the gas has no effect on cataphoresis of a bubble—i.e., does not influence the double electric layer. It follows that we cannot regard the double electric layer as the seat of most of the charges found by Kelvin, Maclean and Galt. But we have seen that the moving bubble carries with it (*a*) an adsorbed gas layer and (*b*) a water layer more or less saturated with dissolved gas. To one or both of these two layers we may look for most of the balloelectric charges varying so greatly in the results just quoted.

Fischer(47) showed that water vapour rising through water generates no balloelectricity, a fact which, combined with those just quoted, proves that for the production of charges the gas and liquid phases must be chemically different. Another significant result due to Fischer is that small bubbles give greater charges than large ones for the same total volume of gas passed. This result might have been anticipated ; the pressure in a bubble is $2T/r$, so small bubbles will explode on reaching the surface with violence proportional to their curvature ; and we shall see in Section VIII that violence and rapidity of surface explosion conduce to large charges.

Coehn and Mozer(48) investigated the effect on the charges in the gas (pure oxygen or hydrogen) when various salts, acids, alkalis, etc., are dissolved in the water used. Using only *very pure* liquids, they found that non-electrolytes and solutions of these in water gave only (—) ions in the gas. On adding electrolytes (+) ions appear, and as the concentration of electrolyte increases these (+) ions increase in number, till the (—) and (+) occur in equal number, and further concentration leads to a surplus of (+) with reversal of net charge. The effect on the water will thus be that as the concentration of the electrolyte increases its charge will be reduced from (+) to zero and then reversed.

P. J. Nolan(49) finds, in opposition to the principle stated by Coehn and Mozer, that both (+) and (—) ions are produced in the air bubbled through alcohol, and that they are exactly equal in number.

Another result due to Coehn and Mozer is that the charge produced in a pure liquid by bubbling increases roughly in proportion to the dielectric constant of the liquid employed, using the same gas.

Coehn and Duhme(50) have quite recently investigated the bubbling of gases through various liquid metals. In the important case of pure mercury the net charge escaping into the gas is (+), but if traces of certain metals be added the sign of the charge is (—). In the case of gas bubbled through water the gas, as we know, receives a (—) charge.

Lenard, from a consideration of Becker's work, considers that the ions in the air which has bubbled through mercury are small mercury droplets. But McClelland and P. J. Nolan(51) find that they are water droplets, and can be eliminated when the materials used are well dried.

Extensive research by McClelland and P. J. Nolan(51) and by J. J. Nolan(52) on the bubbling of air through mercury and alcohol reveals the existence in the gas over the liquid in both cases of groups of ions of definite mobilities. According to this remarkable result, there appears to be a unit stable charged water globule, either (+) or (—). This unit would have maximum mobility (+) or (—). But if two (+) units coalesce with one (—) unit, there results a globule of the unit (+) charge, but of three times the mass, and hence of mobility one-third that of the unit globule. By coalescence in more complex ways, there arise globules of one-fifth one-seventh, etc., mobilities. This result—not yet explained—should throw light on the important question of the mechanism of the explosion. Nolan and Gill(26) find that here, as in Drop-splitting (Section VIII), the charge developed is proportional to the area of new liquid surface created, but for a certain size of globule there are places of check in the curve for charge / area.

As to the nature of the explosion of the bubble, we note that since balloelectricity is manifested by non-electrolytes, we must regard the double electric layer, which is not existent in this case (*see* McTaggart's result, Section II) as not essential for the production of balloelectric charges. As the bubble approaches the surface of the liquid the skin of the latter, and the skin of the bubble, retain their (1) oriented layers, (2) adsorbed gas layers, and (3) double electric layers, so that in the limit there are six layers in all, the rest of the liquid molecules being squeezed out by the rise of the bubble. These various layers break as the gas in the bubble pushes its way through the surface and the charges subsequently found in the gas, whilst undoubtedly springing in part from the double electric layer, when it exists, are also derived from the gas layers and/or the oriented layers. The conclusion derived from Coehn and Mozer's experiments on very pure materials and from the other principles quoted above may be put in the following form :—

Consider a non-electrolytic liquid. Both (+) and (−) components of the molecules hold together tenaceously and refuse to carry a current ; yet in the mechanical process of bubbling, electrons are liberated from the molecules and escape into the gas above ; the molecules, now charged (+), being held firmly in the liquid. On introducing an electrolyte into the liquid, a gas after being bubbled through is found to have acquired (+) as well as (−) ions, the former being derived no doubt from the electrolyte cations. Thus of the four constituents which we have considered, the (+) components of the non-electrolyte molecule is the one which most firmly resists the disintegrating force of the bubble. Reversal of the gas charge to (+) occurs at a concentration depending on the type of salt used. This reversal implies that in concentrated solutions (+) charges escape more easily than the anions and electrons jointly. This direct result is of cardinal importance, and requires interpretation.

VIII. Drop-Splitting.

Two cases, very similar, arise in this section :—

A. The impact on falling drops of a vertically rising current of gas.

B. The impact on falling drops of a horizontally moving current of gas.

The first case has been employed by many experimenters.

A.

Simpson(53), in 1909 and later, established that, for water drops of a certain average size, an uprush of air causes the drops to shatter. No shattering of such drops occurs unless the air speed exceeds 8 metres/sec., but if it does occur the droplets acquire (+) charges and the surrounding air (−) charges. The charge found on the water droplets is about 5×10^{-12} coulombs/c.c.

These results were directly opposed to Lenard's view, expressed in 1892(40), that mere dispersion of water is ineffective in producing charge. But later in 1915(54), in his comprehensive review of the subject, Lenard has modified his theory, and considers that while a *steady* air stream of 8 metres/sec. will not cause splitting and electrification, it will do so if it be sufficiently *turbulent* to give large tangentially-directed forces on the surfaces of the water drops.

Hochswender(55) repeated Simpson's experiments, and in the main confirmed his results. He published photographs of bursting drops, which show that the uprushing air first flattens a drop, then blows it into an inverted cup, and finally blows out the top of the cup and generates the charges in the air and on the droplets.

It will thus be seen that the mechanism of drop-splitting must, in its last stages, closely resemble the bursting of a bubble at the surface of a liquid ; the last stage as in bubbling, being the rending of the films peculiar to the interface.

B.

Experiments were made by J. J. Nolan(56), in which the falling drops encounter not an uprising, but a horizontal, air blast. Doubtless the effect is similar to that produced in the experiments of Simpson and Hochswender, though the relative speed of drops and air, other things being equal, is less ; and though the resulting movement is not vertical but inclined. The drops would in that case be distorted and hollowed out, as in Hochswender's photographs, though in the present case the cup would be turned sideways—not inverted.

Nolan found in the emergent stream of air both (+) and (—) ions, the negative being in excess.

One significant result arising from J. J. Nolan's work is that the total charge on the droplets varies as the new area of liquid formed. Though in later work he expresses doubt as to the exact truth of this law, it appears from his results that, at least for rapid air blasts, this quantitative law is valid. Possibly Nolan's result can be interpreted thus :—

When a new surface is very rapidly formed, some of the electrons left on the "raw" surface are able to escape and charge the air (—). In that case charge varies as new area. When, however, new area is slowly formed, electrons cannot escape, and no charge arises. For intermediate speeds of surface formation, the charge cannot be strictly proportional to new area formed.

IX. SPRAYING.

Research in this Section has been particularly extensive. Faraday(29), working with an Armstrong electrostatic machine, proved that *dry* steam rushing along narrow passages does not produce electrification, but that it is essential for production of charges to have *water droplets* included in the blast. Doubtless these droplets have a rough journey in the narrow passages ; so that both the turbulent steam and the walls of the passages through which they pass will contribute to their decimation. The principle thus established should be borne in mind in the experiments with apparatus like ordinary scent sprayers, with which we now have to deal, since we cannot look to the rush of mere dry gas through the tube to account for any of the charges produced.

Christiansen's(57) method of procedure was unlike that of other investigators on spraying, who have generally carefully avoided splashing. His method combines spraying and waterfall processes ; the liquid was sprayed, and the resulting droplets then met a platinum plate. Investigation on the charges arising in the liquid led to the following classification of the various liquids used :—

I. *Aballisch* : Solutions of salts, acids and bases give feeble charges.
II. *Kataballisch* : Water, and its mixtures with oils, give large (+) charges.
III. *Anaballisch* : Solutions of quinine, aniline, etc., give large (—) charges.
IV. *Hyperballisch* : Mixtures of liquids from classes I, II, III give greater charge than the components used (e.g., KCl and alcohols).

Traube(58) found that those materials, such as fatty acids, alcohols and ethers, which when in solution greatly lower the surface tension of water, are kataballisch or anaballisch, whereas aballisch materials have small influence on the surface tension.

Eve(59) found that tap water, distilled water and ether all give an excess of (—) over (+) ions, the ratio of the two kinds for distilled water being 1·2/1·0. Eve obtained equal numbers of (+) and (—) ions when spraying alcohols (benzyl-, amyl- and ethyl-), acetic acid, and chloroform ; but the actual number found is much greater (two to four times) than for pure water ; in short, these materials are more balloactive. If the distilled water in these experiments were pure and contained no electrolytes it should give only (—), according to Coehn and Mozer (Section VII), but one must remember that spraying is a much more violent process than bubbling, and this may account for the contrasting results of the two processes.

Again, in Simpson's experiments on drop-splitting of water (Section VIII), the ratio of (—) to (+) ions is 3/1 ; this ratio, higher than in Eve's results, corresponding to the less violent method of breaking the water surface.

J. J. Nolan and Enright(60), using water of varying purity, found charges increasing with the purity of the water and with its degree of pulverisation. J. J. Nolan and Gill(61) found that, although inorganic salts diminish the positive charge obtained when distilled water is sprayed, *they do not reverse it.* This is an interesting observation, as it is in sharp contrast with well accepted results of Coehn and Mozer in bubbling experiments (Section VII) and of Lenard, J. J. Thomson, Aselmann, Kähler and Rey, all employing waterfall methods (Section VI). The well attested fact is that, in these waterfall experiments, as more and more salt is added to the water the originally (+) charge on the liquid and (—) charge in the air is gradually reduced, and after a critical concentration, reversal of charge to (—) in water and (+) in air occurs. Nolan and Gill(61) to test this disparity between the spraying and waterfall results, worked the sprayer at much lower pressures than usual, but still found no reversal of sign. But with very strong solutions and extreme pulverisation the reversal appeared. But this result is by no means identical with the waterfall reversal, because in waterfall experiments the critical concentration was very weak (say 0·1 per cent.). There is no doubt about the reality of the reversal in the waterfall method ; and Simpson and Hochswender successfully employed salt to obtain neutral solutions in their work on the theory of thunder-storms.

Nolan and Gill attribute the disparity between results by the two methods to the different speed of the formation and rupture of the water surfaces. In a falling column of water time is allowed for any impurity existing in the water (perhaps an impurity occuring in the salt used) to be adsorbed to the surface, and produce its own balloelectric effect when rupture occurs ; whereas in the process of spraying an absolutely new surface is formed, and then torn so quickly that the process of adsorption is precluded. It appears that we must regard the *mechanism* of rupture of the liquid as of the same importance as the nature of the *materials* used.

Nolan and Gill investigated the reversal of charge produced when certain organic materials are dissolved in water. These materials are sodium glycocholate, sodium oleate, methylene blue, and methyl orange. A very minute amount of any of these suffices to bring about reversal, as had previously been demonstrated by J. J. Thomson(41) for methyl violet.

Nolan and Gill(61) showed that when sodium oleate is present in the water used, its balloelectric effect diminishes as the time of its exposure increases. Solutions three days old show no reversal of charge, however finely they are pulverised. Rayleigh(62) had previously shown that sodium oleate does not change the dynamical surface tension of a water jet if the surface be not more than 1/100th of a second old, the surface not having had time to mature by adsorbing the solute. De Noüy(63), has recently shown that the adsorption of sodium oleate of the concentrations used by Nolan and Gill is a relatively slow process ; thus one millionth part of sodium oleate does not affect the initial surface tension of water, but causes a reduction of surface tension 20 dynes/cm. in two hours.

Spraying is a process involving great dynamic complications. Here the top of a column of water is torn off to an unknown depth and in an oscillating manner by rushing gas. As Nolan and Gill found, difficulties arise at the nozzle, which if not specially and accurately met, give rise to errors. These are (*a*) charging of drops by the "water-dropping" effect of Kelvin; (*b*) electrification by friction of the liquid at the nozzle. Again, results obtained may depend on the construction and material of the sprayer used.

It is no wonder, then, that of all branches of our subject, spraying is the one in which co-ordination of results presents the greatest difficulties.

GENERAL THEORIES.

This summary of the subject would be incomplete without some mention of the theories propounded by Lenard and by Sir J. J. Thomson on Surface Conditions.

In 1892 Lenard published his Contact Theory, but this was replaced in 1915 by a new theory(54). In this he postulated a complex of units in a liquid. In aqueous solutions there may exist together with the ultimate H_2O molecules, (1) polymers H_4O_2, H_6O_3, etc.; (2) complex groups having some molecules acting as solvent, others as solute; (3) ions consisting of a single charge with a family of H_2O molecules attached to it. These complex molecules tend to leave the surface, the forces urging them towards the body of the liquid being deduced from reasoning somewhat like that employed by Laplace in his well-known treatment of surface tension and intrinsic pressure. Further, these forces which urge the molecules as a whole, act on the massive portion of the molecules left at the surface, swinging them inwards, and so producing orientation of the surface mono-molecular layer. Lenard states that at the surface of water and all dielectric liquids there exists an electrical double layer which owes its existence to the molecular forces of the liquid. The outer layer—always negative—is identical with the outer molecular layer of the liquid. The essential of all balloelectric action is—according to Lenard—the separation of minute liquid particles from the outer layer.

About the time of publication of these views, Hardy(16), Langmuir(17) and Harkins(64) were developing, from other lines of reasoning, the modern ideas on polarity of molecules, and the consequent orientation of free surfaces.

Lenard's theory proved satisfactory in explaining balloelectric effects for pure dielectrics, and is in accord with the well-known Gibbs-Thomson adsorption relation. But his theory fails to account for

(1) The influence of the gas used in contact with the liquid.
(2) The increase in balloelectric charges due to rise in temperature of the liquid, discovered by J. J. Thomson(41) and confirmed by Rey(25).

J. J. Thomson(65) in 1914 advanced his theory of intra-molecular ionization. In certain substances termed "polar" the molecules are composed of a (+) and a (−) part, a state of affairs brought about by ionization within the molecule. Polar substances should have (1) large dielectric constant for steady fields; (2) a dielectric constant decreasing rapidly as temperature rises; (3) great dissociating power and tendency to cause ionization in gases. The polar substances are water, ammonia, sulphur dioxide, alcohols, etc.; non-polar are hydrogen, oxygen, nitrogen chlorine, carbon dioxide, carbon tetrachloride, benzene, methane, etc. This classification agrees closely with that of De Broglie(45) for active and inactive balloelectric materials.

SUMMARY.

We have cast our net over a wide area of research on the gas-liquid interface. Those sections of the subject giving the greatest yield are :—cataphoresis of bubbles, bubbling through liquids, drop-splitting, spraying, liquid jets, and waterfall effects. Of these six sections, all but the first are concerned with the measurement of the charges (called balloelectric) arising when a liquid/air surface breaks. From a digest of some ninety published Papers it is possible to state the following general principles :—

(1) As will be seen in the text above, highly conflicting results have been obtained by different observers using various methods for breaking a liquid surface. Methods so unlike in operation as waterfall and spraying of the same materials yield very different charges. Slight differences either in purity of material, or in the time of exposure to the gas of the surface to be broken, may account for this disparity in results.

Controversies rage even on the essentials of this subject up to the present time. No doubt progress would be quicker if investigators could agree to collaborate, using pre-arranged materials and apparatus.

(2) In balloelectric experiments no charge ever arises unless the liquid surface is rapidly increased or torn by the action of a gas, or solid, or other liquid.

When a solid rubs over another solid, as in a triboelectric process, those few molecules of the opposed surfaces which come into intimate contact combine completely, and are then torn asunder as the rubbing proceeds. This tearing process involves electrical separation, one surface gaining a net (—) charge, the other a (+) charge. When a liquid like mercury flows over a solid like glass it does not slide, but rolls, and again we have combination and rupture between opposed molecules, with electrical separation as before. But when a gas passes over a liquid the process is different. Combination occurs, but the gas lacks either the rigidity of a solid or the surface tension of a liquid to cause violent rupture of the surface with electrical separation. But if, as in balloelectric processes, the action of the gas is so violent that it tears apart the molecules of liquid, ions (—) make their escape into the gas, so that both it and the liquid acquire charges.

(3) There are three properties of a gas-liquid interface : Potential Difference, Surface Tension and Balloactivity, which are inter-related. Of these, the first two are definitely measurable ; the last is more difficult to define and measure.

We will attempt to correlate some established results as regards these three properties. Consider the action on water of certain materials :—

(*a*) Certain non-electrolytes (e.g., the alcohols) reduce the thermodynamic P.D., the electrokinetic P.D. and the surface tension, but increase the balloactivity. In like manner some oils (e.g., olive) reduce the thermodynamic P.D. and the surface tension, but greatly increase the balloactivity.

(*b*) Electrolytes, in small amount, reduce the electrokinetic P.D. in the case of multivalent (+) ions, and increase it in that of multivalent (—) ions ; they have little or no effect on surface tension ; they reduce, and in some cases reverse, the balloactivity.

(4) The double electric layer, which is the active ingredient of a bubble in cataphoresis, is not the only layer generating charges in balloelectric experiments. Other layers exist at and about the interface, and the amount of charge generated is a

measure of the suddenness and violence of the rupture of these and of the double electric layer, if existent.

(5) In balloelectric experiments, charges may be looked for either in the liquid or the gas. Of these, the gas phase yields much more detailed information as to charge, sign, and mobility of ions produced. But on measuring the ionic charges in the gas, since recombination of the (+) and (−) ions occurs, time is a factor to be considered. McClelland and Nolan, and others, have allowed for this time factor. An important generalization can be stated : Whereas for so many interfaces we find that the results given by the different balloelectric processes are in conflict with one another, in the case of distilled water/air the results are in qualitative agreement.

In this case the net charge in the air is found to be (—) for (*a*) Bubbling (Coehn and Mozer), (*b*) Drop Splitting (Simpson, Hochswender, J. J. Nolan), (*c*) Spraying (Christiansen, Eve, J. J. Nolan and Gill), and (*d*) Waterfall (Lenard, Kähler, Aselmann). The charge in the water in all these cases is (+).

(6) In researches on this subject temperature is one factor but little exploited so far. Vacuum is another, which has not been employed at all, to our knowledge. Yet in jets, waterfall and bubbling, if in no other cases, a vacuum on the interface is attainable at least in the important case of liquid mercury, and should profoundly influence balloelectric results. There remains a large field for investigation, especially in the direction of the comparison with identical solid, liquid and gaseous materials, of results obtained by the various methods coming within the purview of this Paper.

We wish to acknowledge the kind assistance of Professor J. J. Nolan in the form ot much information supplied by correspondence.

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DISCUSSION.

Dr. P. E. **Shaw** (communicated remarks) : I regret that I cannot attend this meeting. The usefulness of studying the liquid-gas interface, as we have tried to do, appears when we contrast it with the old and troublesome case of the solid-solid interface. Consider a case : Glass-cotton. The surface of each of these solids is strained with a tension tangential, and a pressure, normal to the surface. These strains are indeterminate, they vary with depth from the geometrical surface, and depend on the recent physical treatment and on the action of chemicals used in their cleaning. Above the surfaces of each solid are films many molecules thick, formed by gases, especially water vapour, condensed from the air. In the films themselves are ions proceeding from the solid, which, according to Knoblauch's theory, play a preponderating part in producing electric charges when contact occurs. In addition, the films contain any impurities which may have accumulated on the surfaces. In fact, each solid surface, owing to its attractive forces, becomes a natural dumping ground for any solid, liquid or gaseous materials, which may come near it. If we bring the glass and cotton into contact, the electrical separation which now occurs depends not only on the surfaces themselves, but also on the mechanical pressure applied and on the gas pressure existing at the time. If, further, the surfaces are rubbed together heat is generated, the temperature of each surface changes differently ; the chemical and physical conditions change, and the charges produced vary accordingly. Thus, the surface of a solid is a particularly unfavourable ground for investigation. We have a great number n of unknown factors, and to solve our problem of the genesis of charges, we require n equations. It becomes hopeless to frame a theory until we have acquired a vast amount of data derived from experiments with n changes of the physical and chemical factors involved. But now turn to the liquid-gas case, say, water-air. Both materials can be easily cleaned and renewed, the film trouble is slight or non-existent, and the strain conditions, occurring on the liquid only, are constant and ascertainable. So this interface is relatively easy to investigate. Yet even for this simple liquid-gas case the results gleaned so far are meagre, considering the great number and variety of researches which have been undertaken. But at least one great law emerges, viz., that electrons are liberated from a liquid surface whenever it is violently torn by an alien gas. Interface problems will assuredly bulk very large in the immediate future ; and I think the soundest way to approach the difficult cases is by way of the simple ones, such as the liquid-gas and the solid-gas.

Prof. A. M. **Tyndall** : The subject is full of interesting problems, solutions of which do not seem possible at present. For instance, when air is bubbled through water, many ions are formed, but bubbles of steam from boiling water give none. Mr. V. J. Long, in my laboratory, has recently noticed that neither water nor liquid air boiling violently in an open dish produce any ions. It is difficult to see how any theory which does not introduce the gas phase as an important factor could explain this. Are the authors aware of Aitken's modification of Rayleigh's impinging jets experiment ? It is well known that slight electrification causes the jets to coalesce. Aitken has shown that jets of hot water coalesce without the aid of electrification. Possibly the reduction in the adsorbed layer of air at the higher temperature is the cause of this. If so, could electrification influence the adsorbed content of gas in the same way ?

Prof. F. L. **Hopwood** : It has been shown by Trouton that in the limiting case of a bubble moving through a tube under gravity and filling it the speed is independent of the size of the bubble. Such bubbles have ripples on their surface (investigated also by Gibson), and possibly these ripples if they occur on smaller bubbles might cause the departure from theory mentioned by the author in describing McTaggart's experiments.

Reply to the discussion by Mr. H. W. GILBERT :—In reply to Prof. A. M. Tyndall : As we have noted in the Paper, in all cases when balloelectricity is produced the two phases must differ chemically. Thus, J. J. Thomson showed in 1894 that drops falling through their own vapour produced no balloelectricity, and many other workers have shown similar negative effects for bubbling and boiling processes. This evidence constitutes one of the great objections to Lenard's theory, which ascribes a negligible effect to the gas phase. The explanation of Aitkin's phenomenon may be that at the higher temperature we have a reduced surface tension. Rayleigh has shown that the presence of a minute amount of grease or dust will cause coalescence of the bouncing jets. However, as suggested by Prof. Hopwood, the adsorbed layer of air is also an important factor. It might be possible to test it by some modification of the method used by Palmer to investigate adsorption films on metals (Proc. Roy. Soc., July, 1924). In reply to Prof. F. L. Hopwood : The effect noticed by Gibson and Trouton may be present in cataphoretic experiments like those of Quincke, where the bubble nearly fills the tube. With the small bubbles moving very slowly along the axis of a tube of diameter many times their own the effect would be very slight.

XIX.—A CONTACT THEORY OF DIELECTRIC ABSORPTION AND POWER LOSSES.

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Abstract.

A simple explanation is proposed of absorption, residual charge, and allied phenomena, in solid dielectrics. In accordance with classical theory, the dielectric is assumed to possess a certain true capacity, defined by its dielectric constant, and a certain conductance, here considered as probably electrolytic in type. The so-called anomalous properties are considered to be due to the properties of the contact surfaces of the dielectric and its metal electrodes. These contact surfaces appear to offer great resistance to the passage of ions or electrons across them, so that when an E.M.F. is applied to a condenser there is an accumulation of charges at the surfaces. These charges constitute the absorbed and residual charges. The experimental evidence in favour of this hypothesis is reviewed.

The behaviour of each contact surface is such that it may be represented by a large capacity in parallel with a high resistance, and thus a capacity-resistance combination is suggested, which is equivalent to an actual condenser. The properties of this combination are considered mathematically with reference to the experimental laws of absorption, and power losses in alternating fields, and the extent to which the theory is capable of explaining the experimental results is pointed out. The bearing of the theory on dielectric measurements under various conditions is also discussed.

I. Introductory.

Some time ago Mr. D. W. Dye and the author made a series of measurements on the dielectric properties of insulating materials in thin sheet form, using two different methods: (1) The change in capacity produced on inserting the material in the air gap between two parallel metal plates was measured; (2) Mercury electrodes were applied to the two sides of the sheet under test, and the capacity of the condenser so formed was measured. Values of the dielectric constants of the materials calculated from the results of these observations by the usual formulæ showed systematic discrepancies, the mercury electrode method always giving the higher result. In order to explain these discrepancies, the writer put forward the suggestion that there was a contact effect at the mercury dielectric surface—e.g., a "polarisation capacity" effect such as occurs in electrolytes. We have considerable reason to believe that the discrepancies quoted in our paper on mica(1) are largely due to slight irregularities in the construction of the air gap; but experiments made on varnished cloth did not seem to be sufficiently explained by these irregularities, and led to the examination of the possibilities of the contact effect suggested above. It now appears that such an effect is capable of explaining many of the anomalous properties shown by dielectrics, and it is proposed to show in this Paper how the phenomena of the polarisation of dielectrics under a constant electrostatic field (i.e., the so-called absorption phenomena), the residual charge, and the power loss in alternating fields are all capable of explanation in terms of conduction, electrolytic in type, in the body of the insulating material associated with what may be described as a contact capacity effect at the surface.

II. POLARISATION IN CONSTANT FIELDS.

According to Maxwell's theory, the electric actions occurring in any material are characterised by two constants, the specific conductivity of the material and its specific inductive capacity or dielectric constant. If an electrostatic field of strength F is applied to a substance, electric displacement of two kinds is produced:—

(*a*) There is a continuous movement of electricity in the direction of the applied field. If σ is the specific conductivity of the material, then the quantity of electricity crossing each unit of area of the substance (the area being measured at right angles to the flow) in each unit of time is given by $F\sigma$. This displacement of electricity continues at this constant rate as long as the field F is maintained.

(*b*) Another kind of electric displacement is also produced. This does not increase indefinitely with time, but almost instantly reaches its final value of $KF/4\pi$, where K is the dielectric constant of the material.

At the present time we associate the electric displacement of the first kind with the free electrons in the material, and that of the second type with the electrons which are bound by " elastic " forces to the fixed nuclei of the atoms or molecules.

It is a matter of common knowledge, however, that when a condenser is made by applying metal electrodes to any dielectric which is partly or wholly solid, and an E.M.F. is applied to these electrodes so as to maintain a constant potential difference between them, the actual electric displacement occurring is not simply that indicated by this simple theory. What is actually observed is—

(1) A comparatively large instantaneous* electric displacement. This is considered to be the Maxwellian displacement $KF/4\pi$.

(2) A further electric displacement, not at a uniform rate such as would identify it as the displacement due to conductance $\int F\sigma dt$, but at a rate which decreases with time, rapidly at first but afterwards more slowly, until finally (after many hours or even months) the rate of displacement becomes very small and constant, so that it is then assumed to be the conductance displacement, and is used to determine the constant σ. In other words, what is actually observed is—

(1) The true capacity charge, regarded as instantaneous.
(2) The " absorption " current, which gradually diminishes to zero, leaving only
(3) The leakage current.

The simple classical theory further says that when the field F is removed the displacement $KF/4\pi$ vanishes, and the conductance displacement ceases—i.e., there is an instantaneous displacement $-KF/4\pi$, which is the collapsing of the capacity displacement caused by the initial application of the field, and is thus in the opposite direction to the original displacement, and no further conductance displacement takes place. Thus the displacement due to capacity is regarded as reversible; that due to conductance is not. In practice, if the condenser which we have imagined to be charged is short circuited by connecting its plates together by means of a wire, what is observed is—

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* Following the usual custom in discussing dielectric anomalies, the actual electric displacement is regarded as consisting of two portions, one being regarded as instantaneous, and the other being a function of time. This division is, of course, quite arbitrary, and is simply adopted as a matter of convenience. An exact specification of the " instantaneous displacement " is probably impossible. A masterly discussion of the matter has been given by Hopkinson, Phil. Trans., Vol. 189, p. 109 (1897).

(1) An instantaneous displacement equal in magnitude to the original instantaneous displacement, but of opposite sign.

(2) A further displacement also in the opposite direction to the original displacement. This occurs at a rate which is rapid at first, but which gradually decreases down to zero.

In other words, we observe the true capacity discharge, regarded as instantaneous, followed by the residual discharge current, which gradually decreases down to zero, leaving the condenser entirely uncharged. If, however, the condenser is open circuited (i.e., the wire connecting its plates together is removed) before the discharge is complete (and this may take hours or days), a charge gradually builds up on the electrodes or plates of the condenser. This is known as the residual charge, and is of the opposite sign to the charge taken by the condenser during the time the voltage is applied.

Several theories have been put forward in order to explain these anomalous charging and discharging currents in a dielectric—i.e., currents or electric displacements which are apparently independent of the conductance σ and the dielectric constant K. Maxwell showed that they can be explained without any extension of the simple theory if the dielectric be assumed to consist of layers of materials of different properties, so that σ/K is different for different layers. This explains the existence of the phenomena in the case of a cable with a laminated dielectric; but H. A. Wilson(2), Thornton(3) and S. W. Richardson(4) have found the same effects in crystalline quartz and iceland spar, and it is difficult to believe that such substances are not of homogeneous structure in any particular direction. Von Schweidler(5) has advanced the theory that a dielectric possesses in addition to the displacement electrons, which are capable of vibratory motion about their equilibrium position with a definite natural period and definite damping, other ions in which the damping is so great that their motion is aperiodic. These ions move very slowly under the influence of an applied field, and thus give rise to the anomalous charging current. The anomalous discharge and the residual charge are of course considered to be due to their slow return to their original equilibrium positions. Unlike Maxwell's theory, this assumes that the dielectric actually possesses other properties than those characterised by σ and K, a complication which one would prefer to avoid if possible. Thornton(3) has supposed that the anomalous displacement is due to a further motion of the displacement electrons, due to inter-attractive forces consequent upon their initial displacement by the applied field. It is, however, difficult to see why this further displacement should take place so slowly and why it should exceed in amount the displacement predicted by the Lorentz theory of dielectrics, which it must do, if it is to explain the observed facts.

The theory now proposed is as follows: The dielectric is assumed to possess the two properties defined by K and σ only. Thus it possesses a definite capacity and a definite conductance. The conductance is considered to be electrolytic in type. In Fig. 1 let AB represent two metal electrodes applied to a slab of dielectric. In practice the metal will probably never actually make true contact with the dielectric, such contact as one gets in a soldered joint, for example. In the diagram therefore, a small gap is shown between the dielectric faces CD and the metal faces AB. Now suppose A and B are connected to a battery so as to maintain a constant potential difference between them. The first consequence is that a potential gradient is set up in the dielectric and thus a definite displacement proportional to the dielectric constant of the material K is produced. This is the normal charge. Now, as a consequence of the potential gradient in the dielectric, the ions begin to move so as to produce a current proportional to its conductance. The positive ions move towards C, say, and the negative ones towards D. Corresponding to this current in the dielectric, there must of course be a movement of free electrons in the connecting leads to A and B. Thus we have a further accumulation of negative electrons at A, whilst positive charges preponderate still further at B, as shown. The theory now supposes that these ions at the contact faces find great difficulty in crossing over from dielectric to metal, and opposite charges continue to accumulate. It is obvious that these charges at C and D cause a back E.M.F. in the dielectric, which tends to stop this current. Thus, this so-called anomalous current, or absorption current, gradually gets smaller and smaller, until finally the number of ions moving up to C and D is only equal to the numbers which can cross the contact face. This is the current which is usually regarded as the true leakage current. According to this theory, when the condenser has reached this steady state (and thus from the usual point of view is fully charged) the actual potential gradient in the dielectric is comparatively small, and therefore the true dielectric displacement is small. The charge on the contact faces may, however, be very great, and this is considered to be the " absorbed charge." The voltage distribution in this case is shown in Fig. 1 (*b*).

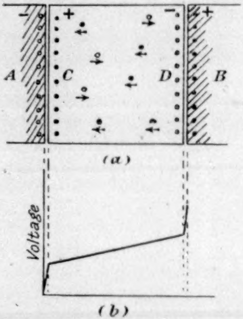

FIG. 1.—DIAGRAMMATIC REPRESENTATION OF A DIELECTRIC WITH METAL ELECTRODES.

Consider now the phenomena of discharge. Let the plates AB be connected by a wire so as to equalise their potentials. Let V be the voltage applied to A and B in the first case, and let x be the thickness of the dielectric, and $x+\delta x$ the effective distance between A and B—i.e., the effective thickness of the dielectric when allowance is made for the imperfect electrode contact. When the condenser is charged, let P be the back E.M.F. due to the accumulation of ions round C and D, then the resultant voltage gradient in the dielectric is $\frac{V}{x+\delta x}-\frac{P}{x}$, and the true dielectric displacement is $\frac{K}{4\pi}\left(\frac{V}{x+\delta x}-\frac{P}{x}\right)$. On short circuiting, V is reduced to zero, and thus the voltage gradient becomes $-\frac{P}{x}$, and the true dielectric displacement $-\frac{K}{4\pi}\cdot\frac{P}{x}$. Thus the change of displacement on short circuiting is $-\frac{K}{4\pi}\left(\frac{V}{x+\delta x}\right)$. This is seen to be equal to the instantaneous charge on applying the voltage V, and thus agrees with experiment. The surprising conclusion arrived at, however, is that this instantaneous discharge, although equal to the instantaneous charge, is not merely the collapsing of this charge. In fact, instead of becoming discharged on short circuiting, the dielectric becomes charged or polarised in the opposite direction by the back E.M.F. due to the ions at the contact surfaces. Another effect of this E.M.F. is that now the ions begin to move in the opposite direction, thereby causing the "anomalous" discharge current. The greater the extent of this motion the smaller becomes the back E.M.F. of the ions, and thus this current gradually decreases down to zero. If the external circuit is not kept closed, the movement of the ions away from the contact surfaces liberates corresponding charges in the electrodes, and as these charges cannot flow along the connecting wire and so neutralise each other, there is an accumulation of electricity on the condenser plates. This is the residual charge. It may be large enough to raise the condenser plates to quite high voltages.

Curie(6) and S. W. Richardson(4) have discussed their observations on dielectrics in terms of an "internal E.M.F. of polarisation." On the present theory this is simply the back E.M.F. due to the accumulation of ions at the contact faces. Richardson made measurements of the E.M.F. by means of a potentiometer arrangement. He found that P, the internal E.M.F. of polarisation, increased with the time of charging, rapidly at first, but afterwards more slowly, which is, of course, exactly what is to be expected by the theory.

III. THE EQUIVALENT CAPACITY-RESISTANCE COMBINATION.

It is to be noted that the metal-dielectric contact surface behaves like a large condenser, and according to this ionic theory, these "contact condensers" must be supposed to possess very high resistance. Thus we may represent the actual condenser by the combination of perfect condensers and resistances shown in Fig. 2. Here C_1 represents the true capacity of the dielectric, and R_1 is its true resistance, C_A and C_B are the contact capacities, and R_A and R_B the contact resistances. In all dielectrics in which the "absorption current" greatly exceeds the leakage current we must suppose that the true resistance of the dielectric is much less than the contact resistances.

When this theory was first conceived this assumption was so contrary to current notions that it appeared to be a very serious objection. It means, for example, that if two samples of the same dielectric, but of different thickness, are taken and placed between metallic electrodes (tin foil or mercury, etc), and their resistances are measured in the usual way, then, in spite of the differences in thickness, the resistances will be very nearly the same, since nearly all the resistance lies in the contact surfaces. There seemed to be no experimental evidence for this, and all determinations of the specific resistance of dielectrics tacitly assume that such is not the case. Quite recently, however, H. M. Barlow(7). following a totally different line of investigation, has arrived at the same conclusion—at least for certain insulating materials used in practice. Barlow was investigating the frictional forces between sliding surfaces across which a direct current was flowing. He found that when one of the substances in contact was a metal, and the other a comparatively poor insulator, such as slate, celluloid, red fibre, or lithographic stone, the passage of a current across the contact caused a considerable increase in the frictional force between the surfaces. He traced this to an increase in the normal pressure between the surfaces due to the fact that nearly the whole of the potential drop between the two substances was located at the contact surface—i.e., the contact resistance was enormously greater than the body resistance. In other words, the increase of normal pressure is due to the attractive force between opposite charges, which accumulate on either side of the contact surface, exactly as the present theory indicates. These poor insulators naturally show the effect to a much greater extent than the best dielectrics. Barlow did not detect it in the case of glass, ebonite, or micanite; but in such cases the surface charges would take hours to reach anything approaching their maximum values, and the effect might very easily escape detection on this account. Barlow also quotes some results of Addenbrooke(8) for the direct-current resistivity ρ of celluloid obtained by measurements on sheets of different thickness, using mercury electrodes. Different values are obtained for different thicknesses, in fact ρ is very nearly inversely proportional to the thickness, which again points to the fact that the bulk of the resistance is in the contact face. Barlow himself made measurements of a similar nature on slate and lithographic stone, with the same result. Further, the work of Richards(9) shows that charges of opposite sign can exist on either side of a contact surface of metal and dielectric, even when these surfaces make wringing contact, each surface being flat to half a wave-length of sodium light. Richards measured these charges in the case of contacts of glass, quartz, fluorite and ebonite with steel. (In this case the charges were produced merely by wringing the surfaces together, and thus they may be of a different nature from those produced as a result of the application of an external E.M.F.)

In view of all this, we see that the assumptions required by the present theory are not without some experimental foundation.

IV. SOME PRACTICAL CONSIDERATIONS.

It has been remarked that the contact surface of metal and dielectric may be regarded as acting like a large condenser of high resistance, and that in practice a condenser may be represented by the combination of ideal capacities and resistances shown in Fig. 2. Consider now some of the simplest properties of the system, and it will be found that many obscure phenomena met with in practice are easily explained.

(*a*) *A.C. and D.C. Resistance.*—It is a well-known fact that the alternating-current resistance of a dielectric is enormously less than the direct-current value. By reference to Fig. 2 it is at once seen that the D.C. resistance is $R_1+R_A+R_B$, since the final constant leakage current must traverse these three resistances. If now alternating voltage is applied, the current can cross the surfaces as a capacity current through the condensers C_A and C_B, and since their impedances at the frequencies met with in practice are much less than those of R_A and R_B, the current (R.M.S.) passing in this case for a given applied voltage (R.M.S.) is much greater than the current flowing when unidirectional voltage of the same magnitude is applied. This current now divides between R_1 and C_1, and since R_1 is much less than R_A, the portion passing through R_1 is quite considerable, the actual amount depending on the frequency. Thus the power dissipated in the resistance R_1, with a given applied voltage, is far greater for A.C. than for D.C. Thus may arise the so-called dielectric hysteresis loss, as a consequence of which the effective A.C. resistance is much less than the effective D.C. resistance, though, according to the present theory, if the true resistance R_1 could be measured it would be the same in both cases.

(*b*) *The Absorbed Charge.*—When measurements of the so-called absorbed charge are made, the most astonishing point is its enormously high value—e.g., in an experiment made by the writer, a sample of varnished cloth with mercury electrodes was charged for about 48 hours with a constant voltage. The electrodes were then short circuited, and the residual discharge current measured during the next 48 hours. The total charge coming out in this time (obtained from the time integral of the current) was such as would correspond to a dielectric constant of 700 for the sample. The measured dielectric constant at 800 ∿ was 4·7. Thus the absorbed charge must be hundreds of times greater than the true capacity charge.

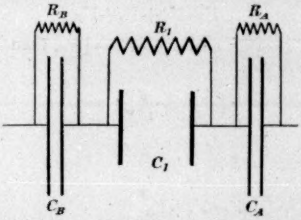

Fig. 2.—Network Equivalent to an Imperfect Condenser.

This is explained by the enormous values of the contact capacities C_A and C_B compared with C_1. When the final steady state is reached the potential drop across C_A and C_B is considerable, owing to the high values of R_A and R_B, and thus these condensers possess very large charges, which are slowly released as the discharge proceeds.

V. Approximate Mathematical Theory.

Before using the model of Fig. 2 for the purpose of calculation it is necessary to note the assumptions involved. It means that the back E.M.F. due to the accumulation of ions at the surfaces is proportional to the total accumulation of charge of either sign—e.g., the back E.M.F. at surfaces $AC=M\int idt$, where $M=\frac{1}{C_A}$ and $\int idt$ is the total charge conveyed to the surface C by the positive ions moving up to it (Fig. 1). If now the direction of the applied E.M.F. be reversed, then negative ions will move up to the surface C, and in this case we have the back E.M.F.$=M'\int idt$. The contact capacity is now $C_A'=\frac{1}{M'}$. It is not necessarily the same as in the previous case, since it may depend on the size of the ions moving up to the surface, and the positive and negative ions may not be of the same size. Similarly, the new contact resistance R_A' may have a different value from R_A, since the positive and negative ions may not move with the same velocity under the action of a given force. The same thing may also occur at the other contact surface, so that when the condenser is carrying alternating current the values of the contact resistances and capacities may also alternate. As a first approximation we shall, however, make the assumption that the contact resistances and capacities are constant for varying applied E.M.F.'s, and, further, that they are the same at the two surfaces—e.g., suppose we are dealing with a parallel plate condenser, the electrodes being of the same size and of the same metal, then by symmetry the two surfaces may be regarded as possessing identical properties.

In Fig. 2 let V_A be the potential differences across C_A, V_1 that across C_1, and V_B that across C_B. Let I be the total current in the circuit. Then

$$I=\frac{V_A}{R_A}+C_A\frac{dV_A}{dt}=\frac{V_1}{R_1}+C_1\frac{dV_1}{dt}=\frac{V_B}{R_B}+C_B\frac{dV_B}{dt}$$

Also, if V is the total potential difference across the condenser

$$V=V_A+V_1+V_B$$

If now we assume $C_A=C_B=2C_0$, and $R_A=R_B=\frac{1}{2}R_0$, then $V_A=V_B=\frac{1}{2}V_0$ (say), and the above equations reduce to

$$I=\frac{V_1}{R_1}+C_1\frac{dV_1}{dt}$$

$$I=\frac{V_0}{R_0}+C_0\frac{dV_0}{dt}$$

$$V=V_1+V_0$$

Here C_0 may be regarded as representing the total contact capacity effect, R_0 the total contact rsistance effect, and V_0 is the total surface voltage drop. These equations are seen to be of exactly the same form as those given by Maxwell's theory of a stratified dielectric when only two layers are considered. The general solution has been given in a convenient form by Meyer(10). It may be written

$$I=C\frac{dV}{dt}+\frac{V}{R}+\frac{kC}{T}e^{-\frac{t}{T}}\int_{-\infty}^{t}e^{\frac{t}{T}}\frac{dV}{dt}dt. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

$$C=\frac{C_1C_0}{C_1+C_0}$$

$$R=R_1+R_0$$

$$T=\frac{R_1R_0(C_1+C_0)}{R_1+R_0}$$

$$k=\frac{(R_1C_1-R_0C_0)^2}{C_1C_0(R_1+R_0)^2}$$

Consider now the cases of most importance in practice.

1. Let a constant voltage E be applied at the instant $t=0$. Then the current is given by

$$I=\frac{E}{R}+E\frac{kC}{T}e^{\frac{t}{T}} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (2)$$

It may be regarded as consisting of two parts, the leakage current $\frac{E}{R_1+R_0}$, and the "after effect" or absorption current$=E\frac{kC}{T}e^{-\frac{t}{T}}$, which decays exponentially with time, approaching a zero value as the value of t becomes very great.

Suppose now that having kept the applied voltage constant for a time interval t_1, it is reduced to zero by short circuiting the condenser. In the interval $t=0$ to $t=t_1$ the current is given by (2). For values of t greater than t_1 it is given by

$$I=\frac{kC}{T}e^{-\frac{t}{T}}\left[E-Ee^{\frac{t_1}{T}}\right]$$

$$=\frac{EkC}{T}e^{-\frac{t}{T}}-\frac{EkC}{T}e^{\frac{(t-t_1)}{T}} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (3)$$

The first term in this expression is smaller than the second, thus the current is now in the opposite direction. Further, the relation between this residual discharge current and the anomalous charging current is exactly that stated in Hopkinson's Superposition Principle(11), which was in agreement with his experimental results.

If the applied voltage E is kept constant for an indefinitely long time, then by integration we see that the total absorbed charge is EkC. If then the condenser is short circuited, the residual discharge current, if t is now measured from the instant of short circuit, is given by $-E\frac{kC}{T}e^{-\frac{t}{T}}$ i.e., the residual discharge current is given by exactly the same expression as was the original absorption current, and if this current is allowed to flow for an indefinitely long time the total charge conveyed by it is $-EkC$—i.e., the absorbed charge is reversible as regards quantity of electricity flowing (though not as regards energy). These relations are borne out by experiment(12).

The term $C\frac{dV}{dt}$ in the general expression for the current through the condenser (1) has not yet appeared in this discussion. It of course represents the instantaneous charge or discharge. C is the "geometric capacity" of the condenser. It differs slightly from the true capacity of the dielectric C_1, on account of the surface capacity C_0, but when C_0 is large (which must be the case if the electrodes make good contact with the dielectric), C and C_1 are practically identical.

With regard to the magnitude of the absorption current, we see that it is directly proportional to the applied voltage and also to $\frac{kC}{T}$. Substituting we find that

$$\frac{kC}{T}=\frac{\left(1-\frac{R_1C_1}{R_0C_0}\right)^2}{R_1\left(1+\frac{R_1}{R_0}\right)\left(1+\frac{C_1}{C_0}\right)^2}$$

which reduces to $1/R_1$ when R_0 and C_0 are so large compared with R_1 and C_1 that the ratios $\frac{R_1}{R_0}$ and $\frac{C_1}{C_0}$ are negligible compared with unity.

Thus in general the smaller the value of R_1 the larger will be the absorption current. This accounts for the rapid increase in the absorption current with rise of temperature.

2. Suppose the applied voltage is alternating and of sine wave form, so that it may be represented by

$$V=V_0e^{j\omega t}$$

where frequency$=\frac{\omega}{2\pi}$

Then the expression for the current becomes

$$\begin{aligned}I&=V\left[j\omega C+\frac{1}{R}+j\frac{\omega kC}{1+j\omega T}\right]\\&=V\left[j\omega\left(C+\frac{kC}{1+\omega^2T^2}\right)+\frac{1}{R}+\frac{\omega^2kCT}{1+\omega^2T^2}\right]\end{aligned}$$

Thus the effective capacity of the condenser at the frequency $\frac{\omega}{2\pi}$ is

$$C_e=C+\Delta C=C+\frac{kC}{1+\omega^2T^2} \quad \ldots\ldots\ldots\ldots \quad (4)$$

where $\triangle C$ is the increase of capacity due to "absorption," and the effective conductance is

$$G=\frac{1}{R}+\frac{\omega^2kCT}{1+\omega^2T^2} \quad \ldots\ldots\ldots\ldots \quad (5)$$

The loss angle δ is given by

$$\frac{C+\Delta C}{C}\tan\delta=\frac{\omega kT}{1+\omega^2T^2}+\frac{1}{RC\omega} \quad \ldots\ldots\ldots \quad (6)$$

The way in which these quantities vary with the frequency is shown in Fig. 3. The effective conductance, and therefore the power loss, is much greater for A.C. than for D.C., and this power loss increases rapidly as the frequency increases. The various functions are of exactly the same form as those given by Maxwell's theory.

VI. Comparison with Experimental Results.

When the formulæ given by this approximate theory are compared with the results of experiments, it is found that, although the capacity, power loss and absorption current vary with time or frequency in a manner which may be roughly represented by the formulæ, the agreement is by no means perfect—e.g., the absorption current for a constant impressed voltage decays rapidly with time, but the actual law of the decay is not a simple exponential one. Similarly, although the capacity and power factor (sin δ, which is practically the same as tan δ) in general both increase as the frequency decreases, the laws are not exactly those shown in Fig. 3. This of course may be explained by the fact that the assumptions made are known to be only approximately fulfilled. As a first approximation we have represented the contact effects by a single capacity C_0 with shunt resistance R_0. We have shown above that they could be better represented by two different capacities $C_A\ C_B$, with corresponding resistances $R_A\ R_B$ (Fig. 2). In this case the equations are of the same form as those for Maxwell's theory of the stratified dielectric with three layers⁽¹⁰⁾. The general solution is found to be of the form

$$I=C\frac{dV}{dt}+\frac{V}{R}+\frac{k_1C}{T_1}e^{-\frac{t}{T_1}}\int_{-\infty}^{t}e^{\frac{t}{T_1}}\frac{dV}{dt}dt+\frac{k_2C}{T_2}\int_{-\infty}^{t}e^{\frac{t}{T_2}}\frac{dV}{dt}dt.$$

where

$$\frac{1}{C}=\frac{1}{C_1}+\frac{1}{C_A}+\frac{1}{C_B}$$

$$R=R_1+R_A+R_B,$$

and $k_1\ k_2\ T_1\ T_2$ are constants similar to those in (1), their values depending on

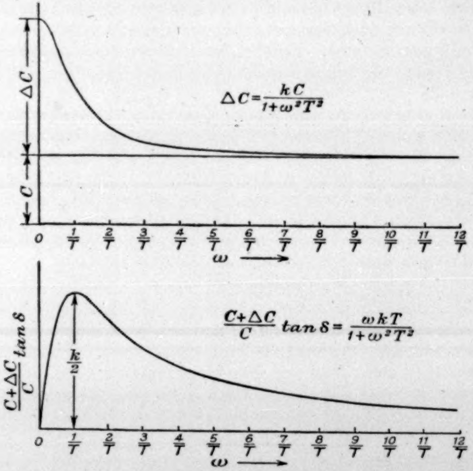

FIG. 3.—VARIATION OF CAPACITY AND LOSS ANGLE WITH FREQUENCY.

the values of $C_1\ C_A\ C_B\ R_1\ R_A$ and R_B. Thus the function representing the decay of the absorption current now contains two exponential terms. The increment of capacity due to absorption is given by

$$\Delta C=\frac{k_1C}{1+\omega^2T_1^2}+\frac{k_2C}{1+\omega^2T_2^2},$$

whilst the expression for loss angle due to absorption is

$$\tan\delta=\frac{C}{C+\Delta C}\left\{\frac{k_1\omega T_1}{1+\omega^2T_1^2}+\frac{k_2\omega T_2}{1+\omega^2T_2^2}\right\}$$

These expressions are capable of representing the experimental results with greater accuracy than the simpler expressions, though, as they are not convenient in use, they are not of much practical importance.

Almost all the theories which have been advanced to explain dielectric anomalies have given expressions of this form—*cf.* those of Maxwell, Pellat, Schweidler. In order to explain the observed facts, Schweidler(5) expressed the absorption current as a function of time by an infinite series of exponential terms, whilst Wagner(13) showed that nearly all the observations could be explained by supposing the time constants of the exponential terms were grouped about certain most probable values, according to a distribution law based on the laws of probability. It is easy to see that the present theory would require extension on the same lines if any set of experimental results were to be explained quantitatively. The contact effects at each electrode have been represented by an ideal condenser, together with a shunt resistance. This can only be a rough approximation. The " double layer " at each electrode is not likely to be of uniform thickness, and thus the surface of the dielectric itself will not be an equipotential surface. Surface conduction will therefore play a small part at least. Further, each element of the contact surface must be regarded as a small condenser with its appropriate resistance, the values changing from point to point over the surface. Each element may give rise to an exponential term in the absorption current time function, and thus this function may be regarded as consisting of a series of exponential terms of varying time constant, the actual value of the time constant depending on the particular element of contact surface concerned. Further, it is quite probable that at any one electrode there would be a certain most probable value of contact capacity and resistance for any element of the contact surface. At the other electrode there might be a similar probable value. Thus the exponential terms in the expression for absorption current might be expected to be grouped about one or two probable values.

Wagner(14) has shown that functions of this form are capable of representing observations he has made on a very wide range of insulating materials. The expressions are of course very complicated, and not at all suitable for calculation in practice. They will not, therefore, be reproduced here. Their value merely lies in the fact that they show how the very complicated results observed in practice may be explained in terms of a theory which in its physical aspect is comparatively simple.

VII. THE AIR GAP AND MERCURY ELECTRODE METHODS.

Experiments on dielectrics are usually made under one of two conditions :—

(1) Metal electrodes are pressed into contact with the dielectric ; or

(2) The dielectric is placed in an air gap between two electrodes.

It may be worth while to examine the two methods in the case of most practical importance (alternating current), in the light of the present theory. In each case we have the dielectric, which we shall consider as a capacity C_1 with shunt resistance R_1, in series with another condenser C_0. In case (1), C_0 is the contact capacity ; in case (2) it is the capacity of the part of the air gap not occupied by the dielectric. We shall suppose the contact resistance R_0 to be infinite. This seems reasonable in the case of the air gap, and in the case of mercury electrodes absorption effects are usually enormously greater than "true leakage," so that the supposition is probably justifiable. Let us consider the capacity at frequency $\omega/2\pi$. We have

$$\text{Capacity}=C+\Delta C=C+\frac{kC}{1+\omega^2T^2}$$

$$\text{where } k=\frac{C_0}{C_1},\ T=R_1(C_1+C_0),\text{ and } C=\frac{C_1C_0}{C_1+C_0}$$

$$\therefore \text{ effective capacity}=\frac{C_1C_0}{C_1+C_0}\left[1+\frac{C_0}{C_1}\cdot\frac{1}{1+\omega^2R_1^2(C_1+C_0)^2}\right] \quad . \quad . \quad . \quad . \quad (7)$$

This represents the observed capacity of the sample in terms of the true constants of the material. Now take the case of the air-gap method. Denote the capacity as measured by this method by C_g. In using the simple formula for this method we put

$$\text{Observed capacity}=\frac{C_gC_0}{C_g+C_0}$$

Equating this with the above expression we find

$$\frac{C_g}{C_g+C_0}=\frac{C_1}{C_1+C_0}\left[1+\frac{C_0}{C_1}\cdot\frac{1}{1+\omega^2R_1^2(C_1+C_0)^2}\right]$$

$$=\frac{C_1}{C_1+C_0}\quad[1+a]\text{ say} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

From this it is obvious that the calculated value C_g is higher than the true capacity of the sample C_1, the difference depending on the factor a. Equation (8) may be written

$$C_g=\frac{C_1(1+a)}{1-\frac{C_1}{C_0}a}=\frac{C_1\left[1+\frac{C_0}{C_1}\cdot\frac{1}{1+\omega^2R_1^2(C_1+C_0)^2}\right]}{1-\frac{1}{1+\omega^2R_1^2(C_1+C_0)^2}}$$

Put $\frac{C_0}{C_1}=z$. Then

$$C_g=\frac{C_1\left[1+\frac{z}{1+(1+z)^2\cot^2\delta}\right]}{1-\frac{1}{1+(1+z)^2\cot^2\delta}}$$

where δ is the true loss angle of the material—i.e., $\cot\delta=R_1C_1\omega$.

This reduces to

$$C_g=C_1\left[1+\frac{\tan^2\delta}{1+z}\right]. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Thus the observed capacity is larger than the true capacity by an amount which depends on $z=\frac{C_0}{C_1}$ and $\tan\delta$. The larger the value of z—i.e., the more nearly the sample fills the air gap—the nearer does the observed value C_g approximate to the true value C_1. If the sample is a poor insulator, then tan δ may be quite large. For a good conductor tan δ approaches an infinite value, and thus the method would give an infinite value for the capacity of such a material, although the true capacity might be quite small.

Now take the case of mercury electrodes, or rather the case in which metal electrodes of any sort are made to give as good a contact as possible with the dielectric, and the contact is then assumed to be perfect. Again the capacity is given by (7)—i.e., if C_m is the capacity given by this method

$$C_m=\frac{C_1C_0}{C_1+C_0}\left[1+\frac{C_0}{C_1}\cdot\frac{1}{1+\omega^2R_1{}^2(C_1+C_0)^2}\right]$$

$$=\frac{C_1\left[1+\dfrac{z}{1+(1+z)^2\cot^2\delta}\right]}{1+\dfrac{1}{z}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

where $z=\dfrac{C_0}{C_1}$ as before. In this case z is known to be very large—e.g., if the dielectric were 1 mm. thick, and there was an air film between electrode and dielectric equivalent to 0·001 mm. of dielectric—then we should have $z=1\ 000$. Thus, as an approximation we may write

$$C_m=C_1\left[1-\frac{1}{z}\right]\left[1+\frac{z}{1+z^2\cot^2\delta}\right] \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

Using the expressions (9) and (11), we may calculate the amounts by which the observed capacity, as measured by these two methods, will differ from the true capacity C_1. The results of such a calculation are given in the following table. The values taken for z are 9 for the air-gap method and 1,000 for the mercury electrode method.

TABLE I.—*Ratio of Observed Capacity to true Capacity.*

δ	tan δ	$\dfrac{C_m}{C_1}$	$\dfrac{C_g}{C_1}$
0° 35′	0·01	1—0·001	1·00001
5° 43′	0·1	1—0·001	1·001
17° 33′	0·316	1—0·0009	1·01
45° 0′	1·0	1·000	1·1
84° 18′	10·0	1·099	11·0
89° 26′	100·0	10·9	1001·0

We see that in every case the air-gap method gives the higher value of capacity. Thus high values of capacity obtained by the mercury method cannot be explained in this way. In the case of a good conductor the value of cot δ approaches zero, and thus the capacity observed by the mecury method approaches the value

$$C_1\left[1+\frac{1}{z}\right]^{-1}[1+z]$$

i.e., C_0, the contact capacity.

Consider now the power factor measurements. The quantity measured by a bridge method is the effective conductance—i.e., $\omega C_e \tan \delta_e$—where C_e is the effective capacity and $\tan \delta_e$ the effective value of the tangent of the loss angle. The value of the effective conductance in terms of what we have considered to be the true constants of the material is given by (5). Thus for the mercury method in the present case we have

$$C_m \tan \delta_m = C \frac{\omega k T}{1+\omega^2 T^2} = \frac{C_1 C_0}{C_1+C_0} \cdot \frac{\omega \cdot \frac{C_0}{C_1} R_1 (C_1+C_0)}{1+\omega^2 R^2_1 (C_1+C_0)^2}$$

$$= \frac{C_1}{1+\frac{1}{z}} \cdot \frac{z(1+z) \cot \delta}{1+(1+z)^2 \cot^2 \delta}$$

Since z is large compared with unity we may write this

$$C_m \tan \delta_m = \frac{C_1}{1+\frac{1}{z}} \cdot \frac{\tan \delta}{1+\left(\frac{\tan \delta}{z}\right)^2}$$

and by reference to (10) we see that this becomes

$$\tan \delta_m = \tan \delta \left[1+\left(\frac{\tan \delta}{z}\right)^2\right]^{-1} \left[1+\frac{z}{1+(1+z)^2 \cot^2 \delta}\right]^{-1} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

In the case of the air gap the test condenser is considered as consisting of two condensers in series, the sample, and the rest of the air gap. Thus the loss angle is calculated from the usual formula for condensers in series, which in the case considered becomes

$$\frac{C_0 \tan \delta_g}{C_0+C_g} = \frac{C_0 \tan \delta}{C_1+C_0}$$

$\tan \delta_g$ being the value obtained for $\tan \delta$ when using the air-gap method. Thus

$$\tan \delta_g = \tan \delta \cdot \frac{C_0+C_g}{C_0+C_1}$$

Substituting from (9) this becomes

$$\tan \delta_g = \tan \delta_1 \left[1+\frac{\tan^2 \delta}{(1+z)^2}\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

We may now use (12) and (13) to calculate the errors in $\tan \delta$ which may be expected in the different cases, exactly as was done for the capacity values. Table 2 gives the values for the cases (*a*) air gap where $z=9$, (*b*) mercury electrodes where $z=1\,000$.

TABLE II.—*Ratio of Observed and True Values of tan δ.*

δ	$\tan \delta$	$\frac{\tan \delta_m}{\tan \delta}$	$\frac{\tan \delta_g}{\tan \delta}$
0° 35′	0·01	$1-10^{-7}$	$1+10^{-6}$
5° 43′	0·1	$1-10^{-5}$	$1+10^{-4}$
45° 0′	1·0	0·999	1·01
84° 18′	10·0	0·909	2·0
89° 26′	100·0	0·090	101·0

Thus the errors only become considerable when the loss angle is very large, and in this case the result obtained with mercury electrodes is the more accurate. With very large power factors, the air-gap method will give too high a value, and the mercury electrode method too small a value. In practice the air-gap method would probably be unworkable with materials of high power factor, owing to want of uniformity of thickness and of perfect flatness in the specimen(17).

Barlow(15) maintains that owing to the high contact resistance it is useless to employ mercury electrodes in order to measure the true volume resistivity of dielectrics. This is probably true in the case of direct-current measurements, but the calculations here given show that when the current is alternating the observed value for the loss angle approximates closely to the true value, even when the contact resistance is infinitely great. In fact, if it is desired to measure the true resistivity of the material, measurements should be made at radio frequencies, for the effective conductivity at frequency $\omega/2\pi$ is given by

$$\sigma=\frac{Ck\omega^2T}{1+\omega^2T^2}$$

When the frequency is very high, so that ω^2T^2 is large compared with unity, this becomes

$$\begin{aligned}\sigma&=\frac{Ck}{T}=\frac{C_1C_0}{C_1+C_0}\cdot\frac{C_0}{C_1}\cdot\frac{1}{R_1(C_1+C_0)}\\&=\frac{1}{R_1}\cdot\frac{1}{\left(1+\frac{1}{z}\right)^2}\end{aligned}$$

And since z is large, this approximates to the true conductivity $\frac{1}{R_1}$. Consider Bairsto's(16) results for slate and marble. He found that as the frequency is raised the effective conductivity rises, rapidly at first, but afterwards more and more slowly, the value becoming approximately constant at the highest frequencies used (of the order 10^6 cycles per second). This value probably corresponds to the true resistivity of the material.

VIII. CONCLUSION.

It is not suggested that all the observed phenomena of absorption, dielectric hysteresis and power loss are due to these contact effects. In the case of substances which are not homogeneous in structure Maxwell's theory applies, and "after effect" phenomena would be observed whether these surface effects existed or not. In the case of quartz, iceland spar, etc., it is difficult to believe that heterogeneous structure provides an adequate explanation of the observed phenomena, and it is here that the present theory provides a more probable explanation. It is significant that there is a lack of agreement between the results of different observers on the decay of the absorption current with time. If this is largely a function of the electrode contact capacities and resistances, then the discrepancies are readily explained.

Another case in which this theory is of importance is that in which the dielectric is of comparatively poor quality—e.g., celluloid, glass and porcelain (especially at high temperatures), porous dielectrics, such as fabrics and paper, slate, and many other insulators of importance commercially. In such cases it is quite probable that the insulator may better be regarded as a solid electrolyte, in which the ionic mobility is small, than as a true dielectric, and the effect of heterogeneous structure may be small compared with the contact effect. Many of the curious phenomena observed in such cases are easily explained by this theory. For example, the large positive temperature coefficient of the effective capacity of porcelain(17) is a direct consequence of the negative temperature coefficient of its resistance, for it is obvious from equation (7) that the smaller the value of R_1 the higher is the effective capacity.

The theory also suggests a possible explanation of the difference between the properties of solid and liquid dielectrics. Liquids do not show residual charge phenomena(5), and when they are subjected to an alternating voltage the power loss is practically independent of the frequency(20). This may be due to the fact that in the case of a liquid (which wets the electrodes) the electrical contact is much more perfect, so that the contact resistance and capacity are no longer of importance. The power loss is simply due to the resistance of the dielectric, and is thus independent of the frequency. In this case the effective capacity is independent of the resistivity of the material, and the temperature coefficient of capacity is negative and not positive, as it is in the case where contact effects are large. Cassie(18) found that the temperature coefficient of capacity was positive for solids and negative for liquids.

A further interesting point which arises is the behaviour of the material under very high voltages. It seems probable that in this case the "contact capacity" would break down, so that the contact resistance would no longer be very great compared with the volume resistance. If this were so, then the "absorption current" would no longer be large compared with the leakage current. The behaviour of solid dielectrics under very high voltages has been studied by H. H. Poole(19). He found that whereas at low voltages measurements of leakage resistance are very difficult owing to the absorption current being much larger than the leakage current, at high voltages this is not the case; but the absorption effect, if it then exists, is completely masked by the leakage current.

Thus there is a considerable amount of experimental work, for which the present theory provides an explanation. Quantitative comparisons are very difficult owing to the complexity of the results, but it is hoped in the near future to carry out an investigation on various dielectrics in order to see how far these considerations can be applied quantitatively.

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DISCUSSION.

Prof. G. B. **Bryan** asked whether the author's theory could explain the well-known experiment in which the metal coatings of a charged Leyden jar are found on removal to be themselves uncharged.

Mr. G. W. **Sutton**: I have not yet had an opportunity of reading the Paper, but from Mr. Hartshorn's explanation of his theory it would seem that there should be a fairly simple and direct test of its validity. If there is a stratum of air imprisoned between the electrodes and the surface of the dielectric this air will become ionised and break down at a definite stress. If the D.C. voltage gradient is as he has represented it graphically on the board such a stress would probably be set up at some quite low voltage, and there should be some discontinuity in the voltage/displacement-current characteristic at fairly low values. It has been shown that some such effect as this does take place in the air-pockets which the process of manufacture is liable to enclose in the stratified dielectric of impregnated paper E.H.T. cables.

Mr. T. **Smith**: According to the author's theory the properties of the dielectric are represented by the middle member of three shunted condensers placed in series. By suitable tests the constants of this condenser and the associated resistance can be determined, and observations of this kind can be made for a series of slabs of the same material of different thicknesses. A test of the acceptability of the theory may be obtained by ascertaining whether the changes in these constants for the several pieces of material are in accord with the differences in their dimensions. Has the author been able to justify his theory by such a series of experiments?

Author's reply: In reply to Prof. Bryan, I think that absorption plays but a very small part in the old Leyden jar experiment. Mr. Addenbrooke (Phil. Mag., Vol. 43, p. 489, 1922) has shown that when the Leyden jar has paraffin wax as dielectric, the metal coatings, on removal, are found to possess large charges, and if they are discharged and the jar re-assembled, the whole arrangement possesses very little charge. This small charge is probably due to absorption, and can be explained by the theory I have proposed. The well-known historical experiment only succeeds when glass is used as dielectric, and even in this case the glass must not be perfectly dry. In fact, the explanation seems to be that the real electrodes of the jar are not the metal coatings, but films of moisture on the glass. The metal coatings merely serve as convenient terminals, the removal of which in no way affects the charges on the actual electrodes, the moisture films. I should not expect the action described by Mr. Sutton to occur to any marked extent in the case of ordinary dielectrics, although such actions do appear to explain the ionisation points of certain cables. Fig. 1 of the Paper is only diagrammatic, and the high resistance property of the contact surface is not necessarily due to air. There is certainly discontinuity in the electrical properties at the contact surface, and I have imagined this to have the effect of a very high contact resistance, but the physical nature of this is by no means obvious. An air film would of course possess a high resistance at low voltages, and would break down at high voltages, but the order of the breakdown voltage of films one or two molecules thick is rather an unknown quantity. It is not likely that the ordinary process of ionisation by collision would occur. On the whole an air film does not seem to supply an adequate explanation. I have had under consideration for some time a series of experiments like those outlined by Mr. T. Smith, since they are obviously of fundamental importance, but owing to pressure of other work I have not yet any results to communicate.

XX.—A MAGNETIC BRIDGE FOR TESTING STRAIGHT SPECIMENS AND AN ANALYSIS OF THE HYSTERESIS LOOP OF COBALT-CHROME STEEL.

By EDWARD HUGHES, *B.Sc., A.M.I.E.E.*

Received March 6, 1925.

(*Communicated by* WM. BENNETT, *B.Sc.*)

ABSTRACT.

The author employs a permeameter resembling that of Iliovici, in which the currents in two coils providing the M.M.F. of a magnetic circuit containing the specimen are adjusted until no magnetic potential difference exists between a selected pair of fixed points on the specimen. In the present apparatus the required absence of magnetic potential difference is tested by bringing up a yoke till its ends abut upon the two points in question : the approach of the yoke should excite no current in a search coil wound on the specimen and connected in a low-resistance galvanometer circuit. Resistance is then added to the galvanometer, and the deflection caused by a reversal of the two magnetizing currents enables the flux density to be calculated. The hysteresis loss per c.c. per cycle in cobalt-chrome steel is found by this method to be approximately $0{\cdot}056\, B^{1\cdot 7}_{\mathrm{max.}}$ ergs, though the index of $B_{\mathrm{max.}}$ actually decreases with increasing values of the flux density. The magnetization curve for this substance, above the point of inflexion near the origin, is found to be given by $\frac{H-c}{\beta}=d+\frac{H}{\beta_s}$, where $\beta=B-H$, β_s is the saturation value of β, and c, d are constants. To form a permanent magnet whose external energy per c.c. is within 5 per cent. of the maximum obtainable, a magnetizing force of upwards of 1,000 C.G.S. units must be applied to cobalt-chrome steel. The βH loop between the $+\beta$ and $-H$ axes is given by a formula similar to that for the magnetization curve, with different constants instead of c, d, β_s, the value corresponding to β_s being much less than the true saturation density.

MANY forms of permeameters* have been devised for testing bars of magnetic material, but most of them involve careful machining to reduce the reluctance of the joints, and require yokes of very low reluctance compared with that of the specimen under test ; and the few permeameters that can be used to determine the hysteresis loop with accuracy are complicated and cumbersome. The magnetic bridge described in this Paper is of simple construction, and enables both the magnetization curve and the hysteresis loop of a bar to be obtained accurately over a wide range of magnetizing force.

Let us first consider a permeameter due to Iliovici, to which the writer's apparatus bears much similarity in that the M.M.F.s of two coils, acting on a common magnetic circuit, are so related that there is no magnetic potential between two particular points of that circuit. Thus, in Fig. 1, AB represents the bar to be tested, having a magnetizing coil NN and a search-coil S ; and CD is a yoke carrying a winding M. An auxiliary yoke Y with search-coil T, is clamped firmly against AB.

After the specimen is demagnetized, switch V is put over to T ; and with a given current through N, that through M is varied until a reversal of the exciting currents does not give a deflection on the galvanometer scale. V is then changed over to S, and the galvanometer deflection is noted for a reversal of the magnetizing currents. The flux density in *AB* for a given M.M.F. between the points of contact of *Y* with *AB* can then be determined. Such a method eliminates the reluctance of the joints from the calculation but it possesses the following disadvantages :—

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* *See* Dictionary of Applied Physics, Vol. II., pp. 474-486.

(1) The portions *CD* and *AB* of the magnetic circuit generally have different time-constants, with the result that the flux in one part grows more rapidly than that in the other ; consequently the galvanometer may give a double kick, moving first in one direction and then in the opposite direction. Under such circumstances, it is impossible to obtain an exact magnetic balance.

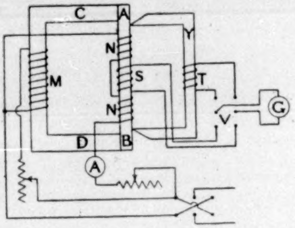

Fig. 1.

(2) Since coil *N* has to be made of appreciable magnitude in order to obtain a magnetizing force up to, say, 400 or 600, it is impossible to avoid a certain amount of leakage flux passing through the search-coil on *Y*, even though there be no magnetic potential between the points of contact of *Y* and *AB*.

(3) The apparatus cannot be used to determine the hysteresis loop, since the relationship between the permeabilities of *AB* and *CD* is not constant.

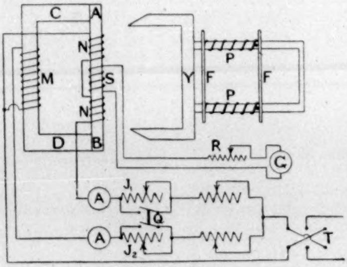

Fig. 2.

These disadvantages have been overcome by arranging yoke *Y* to be carried by two rods passing through guide-plates *FF*, with light springs *PP* to draw *Y* away from *AB* (Fig. 2). Coil *NN* is fixed to the base of the instrument, and made with sufficient clearance inside to allow easy replacement of *AB* by other samples. Search-coil *S* is wound directly on to the specimen under test. With a bar $\frac{3}{4}$ in. by

$\frac{3}{16}$ in. in section, and with 200 turns on S, 6,000 ohms were required in the galvanometer circuit to limit the deflection to a reasonable value when the maximum flux was being reversed. The resistance of the galvanometer and coil S was only 11 ohms; so that with R reduced to zero, the galvanometer was very sensitive to change of flux through S.

Fig. 2 also shows the circuit arrangement for taking a hysteresis loop, the test procedure being as follows:—

With the D.P. switch Q closed and the maximum magnetizing current through NN, the current through M is adjusted until on moving Y up to AB, there is no galvanometer deflection with R at zero. There is no difficulty in detecting an alteration of 1 per cent. of the current through M. It is, of course, essential to reverse the exciting currents several times after each adjustment, so as to ensure the magnetic circuit being in a steady cyclic condition. If the movement of Y up to AB or away from AB causes no change of flux through S, it follows that the points of contact of Y and AB are at the same magnetic potential. After the adjustment has been made, a suitable resistance is inserted into the galvanometer circuit, and the deflection is noted for a reversal of the exciting currents.

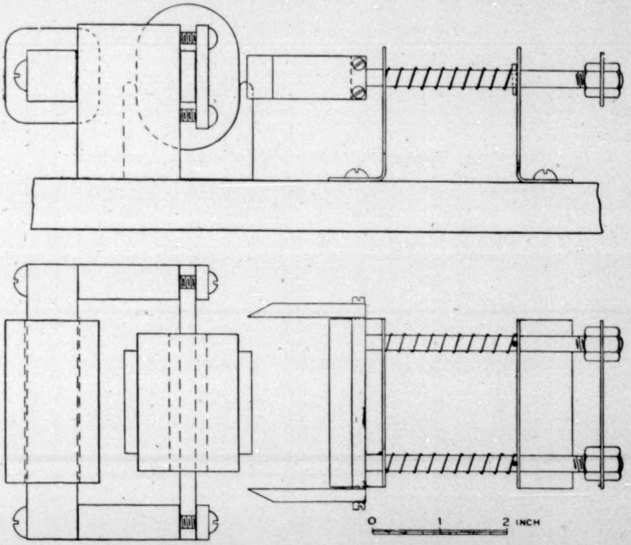

FIG. 3.

Switch Q is next opened, and J_1 is adjusted to give another value of the current. Q is closed, T is reversed several times, and then Q is opened and T reversed simultaneously. The magnetic balance is tested by moving Y towards AB, and J_2 is adjusted to give a balance. As this adjustment may have caused a departure from the correct loop, it is necessary to repeat the test. Usually two or three repetitions enable the correct balance to be obtained.

The test is repeated with different values of J_1 up to infinity, and then with decreasing values but without reversing T.

The apparatus made at the Technical College, Brighton, was designed to test bar magnets 4 in. by ¾ in. by $\frac{3}{16}$ in., and is shown in Fig. 3, the windings being as follows :—

Coil NN =168 turns of No. 18 S.W.G.
,, M =150 ,, No. 18 ,,
,, S =200 ,, No. 36 ,,

Coil NN was wound in six layers with tappings between the second and third and between the fourth and fifth. The three sections were tested separately with the same current, but there was no measurable difference in the magnetic balance nor in the galvanometer deflection. Also the flux through S was measured (*a*) with yoke Y away from AB and (*b*) with Y pressed against AB, but again there was no measurable difference. With the windings immersed in oil, there was no difficulty in working up to a magnetizing force of 600.

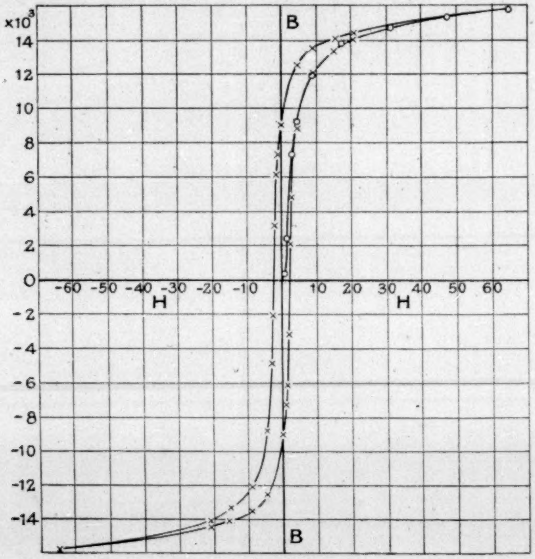

FIG. 4.

In order to test the behaviour of the apparatus with materials of widely different magnetic characteristics, bars of mild steel and of cobalt-chrome steel were used. The circles and crosses in Fig. 4 indicate the points for the magnetization curve and for the hysteresis loop respectively of the mild-steel bar. It is seen that there is no difficulty in obtaining consistent results on the steep portion of the loop even with very low coercive force. It may be of interest to note how closely the magnetization curve of this specimen agrees with the well-known expression of Kennelly* :—

$$\rho_0 = \frac{H}{\beta} = a + \frac{H}{\beta_s},$$

where ρ_0=ferro-magnetic or metallic reluctivity,

β=ferro-magnetic density for a magnetizing force H,

$=B-H$,

β_s=ferro-magnetic saturation density,

and a=a constant.

The values of ρ_0 have been plotted in Fig. 5 for H up to 224 c.g.s. units, the values

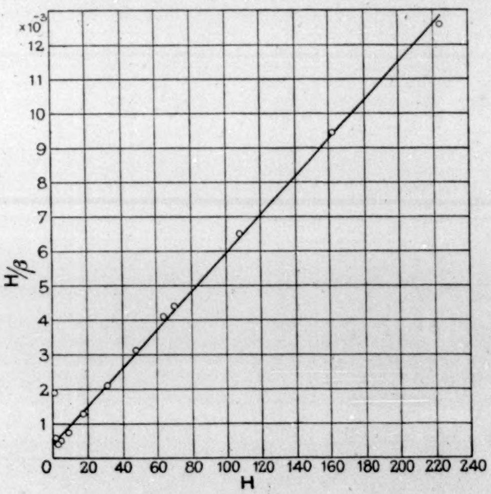

FIG. 5.

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* A. E. Kennelly, "Magnetic Reluctance," Trans. Amer. I.E.E., Vol. 8, p. 485 (1891).

of β_s and a for the line drawn through the mean* of the points being 18,000 and 0·00042 respectively.

Three different qualities of cobalt-chrome steel supplied by the Cobalt Magnet Steel Co., Sheffield, were also tested. Table I gives the composition of these specimens, and Fig. 6, obtained on specimen *B*, is representative of the magnetization curve and hysteresis loops of the three samples.

TABLE I.

Specimen.	C	Cr	Co	Mo	W	Mn
A	1·0	9·72	15·0	1·42	...	...
B	1·0	9·72	9·0	1·42	...	...
C	0·9	5·6	36·0	...	4·0	0·9

The values of the coercive force, the remanence and the energy represented by the different loops taken on the three specimens are given in Table II. Data supplied by the manufacturers showed specimens *B* and *C* to be of the average quality and *A* to be a little inferior to the average specimen of that composition.

TABLE II.

Specimen.	$B_{\max.}$	Coercive force.	Remanence.	Ergs/c.c./cycle.
A	12,300	190	7,600	$50\cdot9\times10^4$
	10,830	181	7,120	$44\cdot35\times10^4$
	8,950	156	5,780	$32\cdot1\times10^4$
	6,840	127	3,960	$19\cdot22\times10^4$
	4,650	86	2,080	$8\cdot535\times10^4$
B	12,520	160	7,870	$46\cdot1\times10^4$
	11,030	156	7,470	$40\cdot45\times10^4$
	9,700	145	6,530	$33\cdot2\times10^4$
	6,800	109	4,040	$16\cdot85\times10^4$
	3,910	70	1,660	$5\cdot51\times10^4$
C	14,740	210	9,620	$68\cdot5\times10^4$
	13,160	200	8,850	$59\cdot65\times10^4$
	10,450	177	6,750	$40\cdot9\times10^4$
	6,950	129	3,620	$18\cdot78\times10^4$
	4,110	64	1,235	$5\cdot43\times10^4$

The hysteresis loss per c.c. per cycle can be represented approximately by :—

$$W = kB^x_{\max.}$$

so that $\log W = \log k + x \log B_{\max.}$

In the lower portion of Fig. 7, $\log W$ has been plotted against $\log B_{\max.}$ for each of the three specimens, and the values of x for different flux densities are given by the upper set of curves. If 1·7 be taken as the mean value of x for the three specimens, the hysteresis loss can be represented fairly closely by

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* The variation in the slope of the reluctivity curve has been discussed by C. P. Steinmetz in the General Electric Review, Vol. 20, p. 135, 1917, and by W. L. Cheney in Scientific Papers, Nos. 361 and 404, of the Bureau of Standards.

$$\text{Hysteresis loss/c.c./cycle} = 0{\cdot}056\, B_{\max}^{1\cdot7} \text{ ergs.}$$

It is of interest to note that the index of B for cobalt-chrome steel decreases with increase of B, whereas for soft-iron and silicon-steel alloys, the reverse effect* has been found by various investigators.

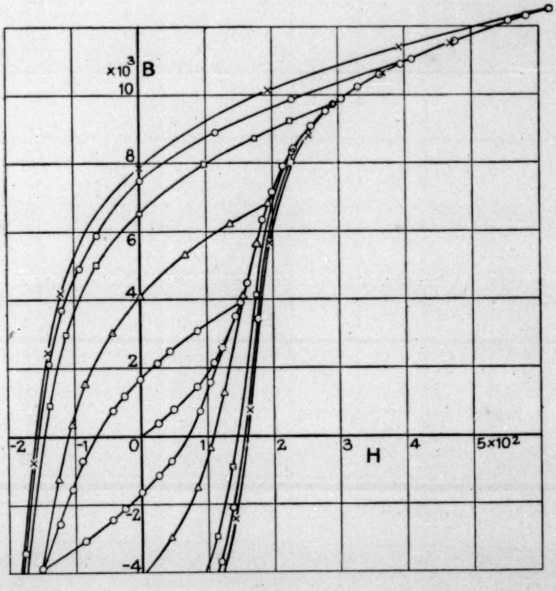

Fig. 6.

Expression for the Magnetisation Curve of Cobalt-Chrome Steel.

The expression for H/β given on page 237 represents very closely the magnetization curve of homogeneous magnetic materials having narrow hysteresis loops and therefore only a small point of inflexion near the origin. When applied to cobalt-chrome steels, however, the above expression requires some modification.* If the magnetization curve forms a part of a hyperbola on *PQ* and *PR*

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* E. Hughes, " Iron Losses in D.C. Machines," Journal I.E.E., Vol. 63, p. 39 (1924).

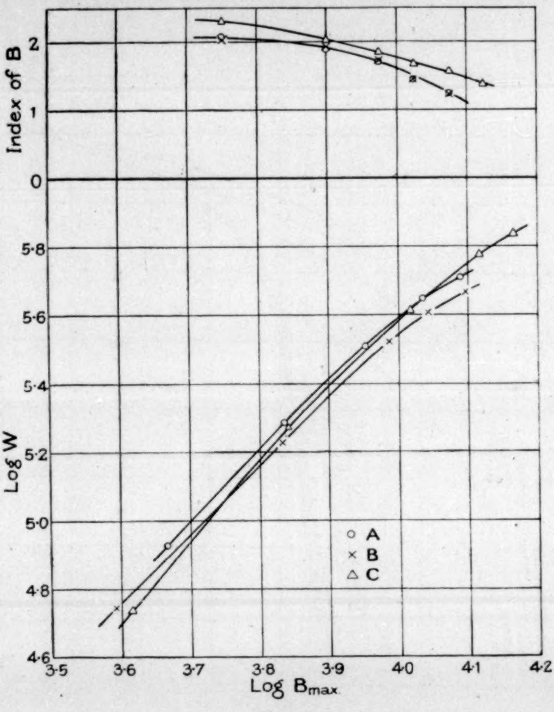

FIG. 7.

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* Curves of H/β for different qualities of steel tested up to very high flux densities are given in the following Scientific Papers of the Bureau of Standards ; but a much greater portion of the magnetization curve determined for each of the cobalt-chrome specimens is in agreement with expression (*a*) derived below than with Kennelly's expression.

(1) W. L. Cheney, "Magnetic Testing of Straight Rods in Intense Fields," No. 361, p. 625, Vol. 15 (1920).

(2) Sanford and Cheney, "Variation of Residual Induction and Coercive Force with Magnetizing Force," No. 384, p. 291, Vol. 16 (1920).

(3) Nusbaum, Cheney and Scott, "Magnetic Reluctivity Relationship as related to certain Structures of a Eutectoid-Carbon Steel," No. 404, p. 739, Vol. 16 (1920).

as axes (Fig. 8), then β_s represents the ferro-magnetic saturation density and we have

$$(\beta_s - \beta)(H - a) = \text{a constant}$$
$$= m \text{ (say)}$$
$$\text{When } \beta = O,\ H = c$$
$$\therefore\ m = b(c - a)$$

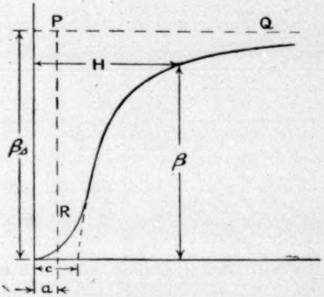

FIG. 8.

Substituting for m and simplifying, we have

$$\frac{H-c}{\beta} = -\frac{a}{\beta_s} + \frac{H}{\beta_s}$$
$$= d + \frac{H}{\beta_s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

where $d = -\frac{a}{\beta_s}$ = a constant for a given specimen.

From the magnetization curves for the three specimens of cobalt-chrome steel the following values have been derived :—

TABLE III.

Specimen.	β_s	c	d
A	13,700	140	—0·00403
B	14,200	100	0
C	15,800	185	—0·00826

Fig. 9 has been plotted with $\frac{H-c}{\beta}$ against H, and the curves show that apart from the initial portion, the magnetization curve follows the hyperbolic law very closely for the range of magnetizing force covered by these experiments.

VARIATION OF $(\beta/H)_{\text{max.}}$ WITH $H_{\text{max.}}$ AND $\beta_{\text{max.}}$.

It is now well known that one of the most important characteristics of a permanent magnet is the maximum value of the product of β and H for

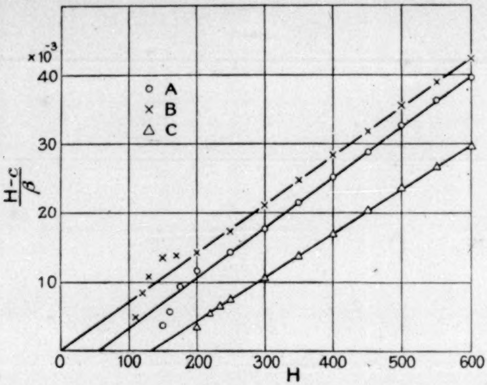

FIG. 9.

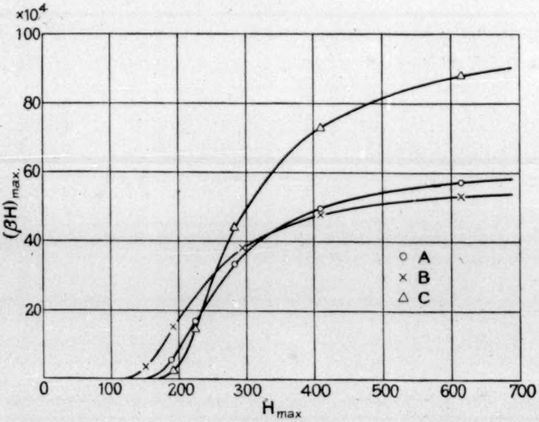

FIG. 10.

that part of the hysteresis loop lying between the $+\beta$ and the $-H$ axes. Thus, Evershed* has shown that the external energy maintained by 1 c.c.

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* "Permanent Magnets in Theory and Practice," Journal I.E.E., Vol. 58, p. 798 (1920).

of magnet steel at flux density β is $\frac{\beta H}{8\pi}$ ergs. The dependence of $(\beta H)_{\text{max.}}$ upon the maximum value of the magnetizing force is seen from Fig. 10, the values being derived from the respective curves of the three specimens of cobalt-chrome steel. It is impossible to extrapolate sufficiently accurately to obtain the maximum value of $(\beta H)_{\text{max}}$ for a given specimen. Consequently Fig. 11 has been drawn with $(\beta H)_{\text{max.}}$ plotted against the value of $\beta_{\text{max.}}$ for the corresponding loop, and the curves are produced to the saturation values of β already given in Table III. Comparison of Figs. 10 and 11 shows that to obtain values of $(\beta H)_{\text{max.}}$ within 5 per cent. of the maximum possible, it is necessary to apply a magnetizing force of at least 1,000 c.g.s. units—preferably about 1,500 or 2,000 units.

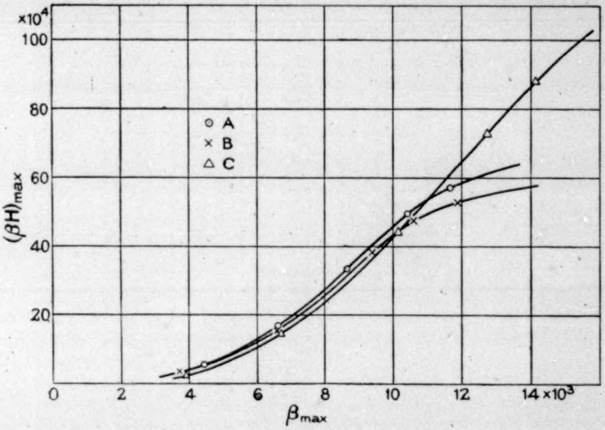

FIG. 11.

From Fig. 11 the following table has been derived :—

TABLE IV.

Specimen.	Maximum Value of $(\beta H)_{\text{max.}}$	Maximum External Energy per c.c. of Magnet.
A	64×10^4	$2{\cdot}55 \times 10^4$ ergs
B	58×10^4	$2{\cdot}31 \times 10^4$ ergs
C	103×10^4	$4{\cdot}1 \times 10^4$ ergs

It has been pointed out by E. A. Watson* that the portion of the βH loop lying between the $+\beta$ and the $-H$ axes is approximately hyperbolic in shape. It can, therefore, be represented by an expression of the form

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* "Permanent Magnets, and the Relation of their Properties to the Constitution of Magnet Steels," Journal I.E.E., Vol. 61, p. 644 (1923).

$$\frac{H-c'}{\beta}=d'+\frac{H}{\beta'_s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

where c'=value of H at which the βH curve cuts the H axis.

$\doteqdot$coercive force,

β'_s=*apparent* saturation density,

and d'=a constant.

The values of c', etc., that give the closest agreement between the above expression and the actual curve between the limits $H=O$, $\beta=\beta_{\text{rem.}}$ and $H=c'$, $\beta=O$, are tabulated below :—

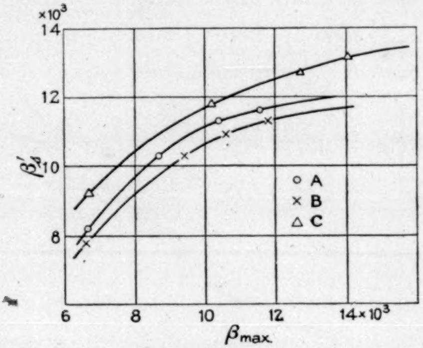

FIG. 12.

TABLE V.

Specimen.	c'	d'	β'_s	Maximum Value of β'_s from Fig. 12.
A	—190	0·025	11,600	12,000
B	—160	0·0203	11,300	11,700
C	—210	0·0218	13,150	13,400

A comparison of the values of β_s and β'_s in Tables III. and V. respectively shows that the saturation density derived from expression (b) is in each case much smaller than the true value given by (a) p. 241, so that the true saturation density of the magnet can not be used to construct that portion of the βH loop that lies between the $+\beta$ and the $-H$ axes.

It may be argued that the values given for β'_s in Table V are not comparable with those for β_s, in Table III, owing to the maximum value of the magnetizing force being only 615. In order to check this argument, the values of β'_s for the different hysteresis loops have been determined, the results being plotted against the maximum values of β for the respective loops. The curves (Fig. 12) have been produced to the saturation densities given in Table III, and the corresponding values of β'_s have been determined and inserted in Table V. They are, however, much smaller than the true values of the saturation density.

CONCLUSIONS.

(1) The magnetization curve and the hysteresis loop of magnetic bars of widely different qualities can be determined easily and accurately by means of the magnetic bridge described above.

(2) The hysteresis loss in cobalt-chrome steel is given fairly closely by

$$\text{Hysteresis loss/c.c./cycle} = 0{\cdot}056\, B_{\text{max.}}^{1{\cdot}7} \text{ ergs.}$$

(3) The magnetization curve of cobalt-chrome steel—apart from the point of inflexion near the origin—is hyperbolic and can be represented by an expression

$$\frac{H-c}{\beta} = d + \frac{H}{\beta_s}$$

(4) The maximum value of the external energy maintained per c.c. of magnet depends greatly upon the maximum value of the magnetizing force applied to the magnet; and to obtain values within 5 per cent. of the maximum possible, a magnetizing force of at least 1,000 units—preferably 1,500 to 2,000 units—must be applied.

(5) The portion of the βH loop between the $+\beta$ and the $-H$ axes is approximately hyperbolic, but the value of β'_s for this portion is much less than the true saturation density β_s of the material. It is, therefore, not correct to utilise the value of β_s when constructing graphically this part of the βH loop of a magnet, nor to term β'_s the saturation density of the material.

DISCUSSION.

Mr. D. W. Dye: The modification of the means of ascertaining that the compensation for reluctance of the joints, etc., of the Iliovici permeameter has been correctly made appears to be a considerable improvement in this apparatus. The use of a movable exploring yoke instead of the reversal of the main magnetizing current is in effect equivalent to the introduction of a statical means of determining that the magnetic potential difference between the two points of contact of Y with the specimen is sensibly zero. The straightness of the reluctivity curves shows that this method of indication is not seriously in error when magnetizing forces greater than about 5 C.G.S. units are in operation. Since, however, the causes mentioned under (2) on page 234 are still operating when the yoke is brought up into contact with the specimen, the condition experimentally realised is actually that the magnetic potential difference between the points of contact of the specimen remains unaltered when the yoke is brought into contact and not necessarily that this magnetic potential difference is quite zero. There will, in general, be a small magnetic potential difference between the ends of Y owing to the leakage field referred to. The effect on the curves obtained by the apparatus will be to shear the hysteresis loop in such a fashion that the coercive force points remain unaltered. This effect still leaves the loop of a normal looking shape and no immediate judgment can be made as to its accuracy. It is when such loops are obtained by a more standard method that the differences are observed; it is doubtful whether the hysteresis loops of materials having a coercive force of 2 C.G.S. units or less can be accurately measured by any type of permeameter in which H is deduced from a magnetizing current and an effective length of specimen which is determined or adjusted to a known value experimentally. The conditions are, of course, very different in the case of materials in the other extreme condition, i.e., hardened magnet steels. An uncertainty in H of 1 unit at the remanence point is of comparatively little importance.

With regard to the statement that the Iliovici permeameter cannot be used in its normal form for hysteresis loop determinations, I am not quite clear why this should be so. If the procedure outlined by the author when using his method is followed except that the search coil T is used, then by whatever means the M.M.F. is provided round the path $A\ C\ M\ D\ B$ of Fig. 1, so long as the final result at the test points of Y is a sensibly zero magnetic potential difference, then the M.M.F. provided by N is that which is expended along the specimen up to the two test points of the yoke Y. The switching procedure by which the desired point on the loop is reached must, of course, be exactly rehearsed when testing by search coil T. The advantages of the movable yoke are, however, so obvious that the point need not be pressed further.

The chief cause of the double throw referred to by the author is, I think, the solidity of the main yoke. In testing apparatus used at the National Physical Laboratory, in which yokes of large section are used, we have found it very advantageous to build them up of laminations of stalloy sheet punched to a suitable shape.

The observations on the variation of the hysteresis index as a function of $B_{\mathrm{max.}}$ are of considerable interest. There is a field for much investigation on this point. The variations of the index appear to be associated with curvature of the BH curve. In the case of the softer magnetic materials, there seems considerable ground for believing that the index rises as B diminishes from a value near to 2,000 to zero, and that it rises again from 12,000 upwards. At values of B above 17,000 or 18,000 it is to be anticipated that the index must fall again since the hysteresis loop is tending to a finite area, although $\mathrm{B}_{\mathrm{max.}}$ may continually increase.

The introduction of the additional constant " c " in the reluctivity equation (a) is an improvement not only in that it enables a greater portion of the βH curve to be represented, but also in that it suggests a line for future work in correlating " c " with the coercive force of the material. this also follows from the experimental fact that the portion of the hysteresis loop between $\beta_{\mathrm{rem.}}$ and $+H_c$ follows very closely the βH curve. We have found this to hold for many kinds of magnetic material. With regard to the author's conclusion as to the high magnetizing force required if the maximum energy per c.c. is to be retained, I have found this to be true for cobalt steels, but for ordinary tungsten magnet steels it is unnecessary to carry the magnetizing force to such high values. A good rule to adopt in tests on magnet steels of all kinds is to carry the magnetizing force up to a value of 8 or 10 times the coercive force expected.

Dr. D. **Owen** suggested that some of the difficulties of Iliovici's method might have been got over by the use of a fluxmeter, but the present method shares with the magnetometer method the advantage that it enables time changes to be observed by applying the test yoke a second time after an interval of fifteen seconds cr so. Had the author examined in this way the question whether a balance when once obtained was permanent for a given adjustment ? Similarly, it would be possible to observe the effect of a rise in temperature. No physical explanation was available for the usual increase of the index of B, in Steinmetz' formula, with increase in the value of B. The author's discovery that in the case of cobalt-chrome steel the index decreases instead of increasing adds another phenomenon to those which must be explained by any theory on this subject.

Mr. **Rollo Appleyard** referred to the fact that in Iliovici's original method two successive kicks of the galvanometer are obtained for each reversal of current, as mentioned by the author. Is this phenomenon to be attributed to the difference in the lengths of the two magnetic paths concerned ? How are the time constants relevant to the respective kicks to be defined ?

Mr. W. C. S. **Phillips** asked how long it takes to carry out a test by this method ? It used to be usual to specify the coercive force and retentivity of magnets, but it was difficult to get rapid determinations for commercial purposes. Had the author checked his results against standard determinations, and what limits of accuracy did he attribute to his method ?

Author's reply (communicated) : Mr. Dye has drawn attention to the effect of leakage flux in the movable yoke Y, and further tests have been made to check the magnitude and distribution of this flux. Six search coils were wound in positions indicated in Fig. A. The bridge was balanced in the usual manner, and galvonometer deflections were noted for the six search coils in turn, when

(a) The exciting currents were reversed with Y at its maximum distance from the core ;
(b) Ditto with Y pressed against the core ;
(c) Y was moved up to the core, the exciting currents not being reversed.

The M.M.F. of coil NN was 1060, and the flux density in AB (Fig. 2) was 8140. The corresponding fluxes in coils 1 to 6 in test (*a*) were 160, 158, 152, 126, 96 and 63 respectively. In test (*b*) it was found practically impossible to obtain reliable figures owing to the double-kick effect referred to in the Paper. Test (*c*) indicated a change of flux of 47, 47, 43, 31, 16 and 39 in coils 1 to 6 respectively. A very definite double-kick was obtained with coil 6, indicating first an increase and then a sudden decrease of flux as Y was moved up to the core. The difference between the fluxes in tests (*a*) and (*c*) gives the flux when Y is held against the core, the flux densities thus calculated for positions 1 to 6 being 141, 139, 136, 119, 100 and 30 respectively, an average of, say, 110 for the whole length of Y. The permeability of the yoke at these low flux densities is not known; but assuming a value of, say, 200, we find that the magnetic potential drop along the whole of Y is $\frac{110 \times 14{\cdot}5}{200}$, namely, 7·97. This figure indicates a possible error of 0·75 per cent. in the value of the M.M.F. acting between the two test-points of specimen AB. Similar tests carried out for other values of the exciting currents confirmed the above conclusion, and also indicated that the leakage flux in Y with the bridge balanced was practically proportional to the exciting M.M.F.

Another method of checking the effect of the leakage flux through Y was carried out thus: The bridge was balanced in the usual manner and the current in M (Fig. 2) noted. Yoke Y was then held against the core, and the current through M readjusted until the net deflection—i.e., the double-kick—in search-coil (1) was as nearly as possible zero. The fluxes in coils (5) and (6) were then found to be in the reverse direction to that when the bridge was balanced by the first method, indicating that the magnetic potential drop in Y had been reversed. The currents in M for the two methods of balancing were 1·93 and 1·91 respectively, so that the error involved in either method is well under 1 per cent. The M.M.F. due to coil NN was maintained constant at 1060.

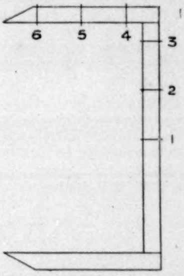

FIG. A.

I do not see why this type of apparatus is not suitable for testing materials having a coercive force less than 2 C.G.S. units, as suggested by Mr. Dye. It has already been stated that the leakage flux in Y appears to be roughly proportional to the exciting M.M.F., so that any error due to that cause should be approximately constant. On the other hand, the elimination of the reluctance of the joints would seem to make this type of apparatus particularly suitable for such work. Further, the yoke—being movable—has its residual flux (if any) reduced to the minimum before it is moved up to the specimen under test. This is a decided advantage over the fixed yoke when very low magnetizing forces are being dealt with.

Mr. Dye is correct in stating that the Iliovici permeameter can be used—theoretically—for determining the hysteresis loop; but the double-kick effect renders it practically impossible with a ballistic galvanometer, though this difficulty could be overcome by the use of a fluxmeter, as suggested by Dr. Owen.

I am interested in Mr. Dye's suggestion that the variation in the hysteresis index may be associated with the shape of the BH curve. It seems difficult to anticipate the behaviour of the index at very high values of the maximum magnetizing force. Both $B_{\text{rem.}}$ and H_c have been shown by Sandford and Cheney (p. 240) to be functions of the maximum magnetizing force ; so that though the area of the hysteresis loop with magnetizing forces of a high order may be increasing very slowly, the value of $B_{\text{max.}}$ will be increasing more slowly. It does not seem, therefore, that the hysteresis index must necessarily decrease.

Mr. Dye's suggestion of a possible relationship between the coercive force and the constant " c " may also lead to useful results. It may be added that since submitting the Paper, equation (1) has been applied to data given in the Papers of the Bureau of Standards referred to on p. 238. For the samples of Honda magnet steel, it was found that the graph based on this equation indicated in each case a definite alteration of slope at a magnetizing force of about 750—a feature that was veiled by the curvature of the ordinary reluctivity graph. The whole of the data in the Paper, with the exception of Figs. 4 and 5, refer to cobalt-chrome steel ; and the high magnetizing force of over 1000 is not meant to apply to other materials.

In reply to Dr. Owen, it may be stated that no effects of time or temperature were noticed. In all the tests the exciting currents were reversed a number of times before taking a reading, and such readings were found to be perfectly consistent and independent of time. For most of the tests the apparatus was immersed in oil, so that the temperature rise of the specimen was very small. When mild steel was being tested at a low magnetizing force, it was found that its flux was disturbed by a sharp impact of Y against the specimen.

Mr. Dye has dealt with the question of the double-kick phenomena raised by Mr. Appleyard. The term " time-constant " refers to the growth of flux in the magnetic circuit concerned, and not to the duration of a kick. It can therefore be defined in the orthodox manner.

With regard to the time taken to determine a single loop, a set of, say, 10 points with three galvanometer readings per point can be taken in about 30 minutes. The remanence itself can be obtained very quickly ; but for finding the coercive force the simplest procedure is to take readings for two or three points in the neighbourhood of zero flux. It may be mentioned that other methods* are available for quick determination of the portion of the hysteresis loop lying between the $+B$ and the $-H$ axes.

No other reliable permeameter was available for testing straight specimens, so that it is not possible to state the limits of accuracy of the present apparatus. It is estimated, however, from a consideration of possible errors, that the net error should not exceed about 1 per cent.

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* Dictionary of Applied Physics, Vol. II, p. 493 ; and Journal I.E.E., Vol. 61, p. 659 (1923).

XXI.—THE EXPERIMENTAL CONTROL OF ELECTRICALLY BROADENED SPECTRAL LINES.

By M. C. Johnson, *B.A.*

Received March 26, 1925,

(*Communicated by* Prof. S. W. J. Smith, *F.R.S.*)

Abstract.

The simple relation to average current density, previously relied upon to describe low-pressure broadening of the lines in a hydrogen discharge, is shown by examination over a finer range to represent a rough approximation only. Concentration of ions is the obvious controlling factor if the Stark hypothesis be adopted ; recombination of ions on this hypothesis may explain the capacity and inductance curves obtained in these experiments, between 0·3 and 1·0 A.U. This view is further tested by controlling the broadening without altering the current in the tube or the period of the discharge ; for this, a range of widening over which rectified and unrectified discharges show lines differing in width, is provided by a magnetic field. The several effects involved in this experiment are accounted for on the theory that line width depends on the number of charges which surround an emitting particle. A thermionic method is also employed for varying the line width, by drawing into the discharge ions additional to those produced by collision.

I. Introduction.

PREVIOUS researches in the control of broadened spectral lines have either been confined to the variation of gas pressure in the discharge tube, or, when electrical conditions have been studied, they have been confined to the variation of the external circuit : work on this method has been published concerning broadening of Balmer lines over a range of 10 to 50 A.U. The present experiments are an attempt further to disentangle conditions, (*a*) by using a range of 0·3 to 1·0 A.U., in which changes hidden in the wider range might be detected, and (*b*) by using other means of control in addition to that of the external circuit ; this latter becomes necessary in view of a recent suggestion that capacity and inductance control of broadening, at apparently constant pressure, acts by a secondary effect on temperature and pressure.

II. Theory.

The first enumeration of the causes of broadening in spectral lines was given by Rayleigh (Phil. Mag., 29, 274, 1915) and Lorentz (K. Akad. Amst. Proc., 18, 134, 1915). The "impact damping" of Lorentz has since been reinterpreted in terms of the quantum theory by Franck (Anniv. Kais. Wil. Ges., 1921), on the basis of Stark's suggestion (Jahr. Rad., 12, 349, 1915), that the electrical fields between atoms should produce a random disturbance of frequency analogous to the Stark-Losurdo separation. Debye (Phys. Zeits., 20, 160, 1919) and Holtsmark (ditto, 162) calculated that fields between ions should be of the right order of magnitude for such an effect. But the most complete experimental evidence for the Stark explanation of low-pressure broadening is that of Merton and Nicholson (Phil.Trans. Roy. Soc., 216, 1916), who found that the intensity distribution curves of certain lines broadened in condensed discharges agreed with the symmetry and other characteristics found in their Stark separation.

Nevertheless, so far, no satisfactory means has been found of varying the experimental conditions so as to isolate any of the factors controlling the interatomic fields which are assumed. The finding of these factors is to be desired, not only as a further confirmation of the Stark hypothesis, but also as throwing light on the conditions in the discharge tube, and in the stellar atmospheres, where low-pressure broadening appears on a scale beyond reach in the laboratory.

The pioneer attempt at such experimental control was due to Rossi (Astroph. J., September, 1914), who used Milner's theory of the spark discharge (Phil. Mag., 24, 709, 1912), and obtanied with it a linear relation between the breadth of Balmer lines of the order of 20 A.U. and the average current density in the spark. Hulburt (Phys. Rev., July, 1923) accepted Rossi's view of average current density as the controlling variable, and suggested grounds for expecting the well-known narrowing with inductance and broadening with capacity. Miss Hanot, on the other hand (Comptes Rendus, 178, March, 1924) suggested that inductance and capacity act secondarily by altering pressure and temperature relations during discharge.

III. EXPERIMENTAL DETAIL.

(*a*) *Apparatus Used.*

The hydrogen tubes used were of the usual Plücker type, except in the case of the thermionic tube described in detail below. Various coils, transformers, breaks, were tried in the search for a steady discharge, and a five inch coil worked on 20 volts with a mercury-motor break was finally adopted. For rectification in certain experiments, a large valve of the recessed-electrode type was employed. Gutta-percha covered high-tension inductances were used, and after trial it was found necessary to replace batteries of Leyden jars by two Lodge-Muirhead oil condensers; the series and parallel combinations of the two halves of each of these provided the variable capacity. The spectrum was analysed by a Hilger constant deviation spectroscope and 20 plate echelon. The magnetic field for the discharge tube involved nothing unusual.

(*b*) *Accuracy of Measurement of Broadening.*

The range of broadening which can be examined by this method is limited by two considerations. Firstly, the region of the spectrum which can be examined in the echelon is never more than twice the order separation, and not much more than 1 A.U. in the case of the present instrument and the line $H\alpha$; secondly, the fine structure of $H\alpha$ covers nearly 0·2 A.U. In the narrow region between these limits, however, the shape of the curve representing the distribution of intensity across the line does not vary greatly. Now the usual definition of "halfwidth" of a line is the distance from its centre at which the intensity has fallen to half its value, so that a measured width depends on the maximum intensity. Two forms have been given (Merton and Nicholson, loc. cit.) of the function which represents this distribution of intensity,

$$I=I_0 e^{-kx^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(i)}$$

$$I=I_0 e^{-qx} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(ii)}$$

where I_0 is the intensity at the centre, I the intensity at a distance x, and k and q are constants. Equation (i) represents the broadening in a gas at low pressure excited by uncondensed discharges, while equation (ii) represents the broadening of each component of the line in a condensed discharge. In the previous work, on a range of 10 to 50 A.U., either of these types of distribution has necessitated the microphotometric measurement of the density of image on a spectrogram. Miss Hanot's accuracy, by this means, is of the order of 10 per cent., while Rossi's accuracy in setting a micrometer wire at the point where the intensity of an image is estimated to be half its maximum value, also reached 10 per cent. In the present experiments, the very much smaller range of broadening used allows the intensity curve to remain very steep and approximately constant, and hence the line is always sharply bounded enough to be measured by a visual method with an accuracy of 4 per cent. (or higher in the most favourable experiments). Accordingly the echelon was fitted with a Watson micrometer eyepiece, a white light comparison spectrum being arranged in the lower part of the field of view to facilitate accurate setting of the wire.

(*c*) *Constants of the Echelon.*

If ε be the difference in wavelength corresponding to the separation of successive orders of spectra,

$$\varepsilon=\frac{\lambda^2}{bt},$$

where t is the thickness of the plates, and

$$b=(\mu-1)-\lambda\frac{d\mu}{d\lambda},$$

in which λ is the wavelength and μ the refractive index of the glass for that wavelength.

For $H\alpha$, $\lambda=6563{\cdot}04$ A.U., and, from data supplied by the makers, $\mu=1{\cdot}5711$ and $t=1{\cdot}0467$ cm. The construction of a dispersion curve for this glass then gave $b=0{\cdot}6039$, whence $\varepsilon=0{\cdot}68$ A.U.

The order separation, from line centre to line centre, was then measured with the micrometer eyepiece, when the position of the echelon gave a turning point in the movement of the orders across the field of view—i.e., when the echelon was normal to the beam of light. This was found to be 38·3 micrometer divisions. To the accuracy of the present requirements, then, 56 divisions represent 1 A.U.

IV. INDUCTANCE AND CAPACITY CONTROL OF BROADENING.

In the work of Rossi (loc. cit.) the average current density in the discharge was taken as

$$\frac{\text{charge on condenser}}{\text{half period of oscillation}}\Big/\text{cross section of discharge},$$

but in the present experiment, with a fixed bore capillary tube, we are concerned only with the average current. If C be the variable capacity, and L the variable inductance, and if we assume the resistance of the tube during discharge is small, the expression above becomes proportional only to

$$\frac{C}{\sqrt{CL}} \text{ or } \sqrt{\frac{C}{L}}.$$

Now the experimental results of controlling the width of the line by inductance (Fig. 1) support a linear relation to the average current, until small values of L are reached—disagreement here may be due to the relative importance at one end of the curve of the inductance in the secondary winding of the coil—but the capacity curves (Fig. 2) do not point to the same relation. These curves each consist of two parts, with a definite discontinuity in direction: experiments which show greater curvature of the lower part show proportionately greater curvature beyond the break, though the latter curvature is always less than the former. Two examples are given in the figure, but the above characteristics occur in all the other series of measurements, made with different types of condenser and discharge. In every case the curvature is opposite to that which would result in a linear dependence of broadening on the average current. This requires explanation, since the period, in the denominator of Rossi's expression, is symmetrically determined by C and L—i.e., so far as period is concerned, capacity and inductance curves should be the same. But the reduction of the expression to $\sqrt{C/L}$ gives a false impression of there being no reason why the regularity of the inductance curve should not also characterise the capacity curve. The following view is suggested as a possible solution of the anomaly; and as leading to a more precise allocation of the responsibility for broadening than to current density.

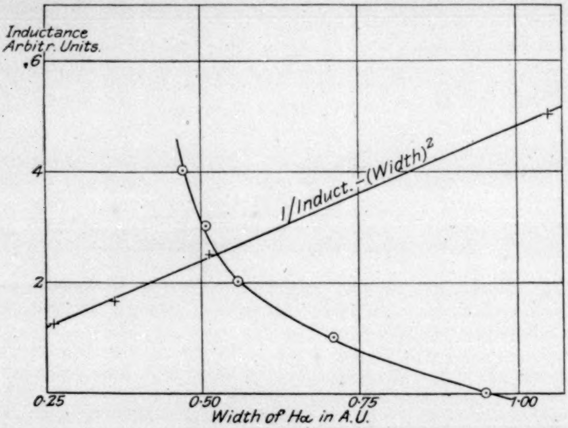

FIG. 1.

On the Stark theory, which is subsequent to Rossi's experiments, the number of charged ions should control broadening. The light is emitted by atoms, the electron of each of which is falling back to its two-quantum orbit; while the dispersion of the single frequency related to this energy charge is due to the field maintained by neighbouring charges. The effective field will be larger when the average current passing is larger, but will be a function of recombination as well as of the production of ions. Now, increase of L increases the period without altering the quantity of electricity discharged at each impulse, and the maximum value reached by the current in each oscillation is decreased. There will be more time for the recombination of ions before the potential next rises to its maximum, and hence a decrease in the field due to the ions which surround the emitting atoms. This will appear as a decrease in line width. Now, increase of C in the denominator of the expression $C/\sqrt{CL}$ acts similarly, but is more than counterbalanced by its increase in the numerator, where it represents a different effect of the capacity, resulting in the creation of more ions than are recombined. Thus we find, on the whole, an increase of the charged neighbours of the radiating atoms. The presence of discontinuous changes of direction in the capacity curves, but not in the inductance curves, is, on this view, a consequence of capacity fulfilling a double function: it can both increase and decrease the number of charged particles, while inductance only decreases their number. Now, the time of reorganisation of an excited atom is known from experiments of Wood and of Wien, and has been calculated by Milne; it is always of the order of 10^{-8} sec. But the time for recombination of a completely ionised atom is a function of a larger number of variables, and though in most cases far greater than 10^{-8} sec., is not determinate. Hence a quantitative relation of the discontinuities in the capacity curve to recombination is not practicable in the present state of knowledge.

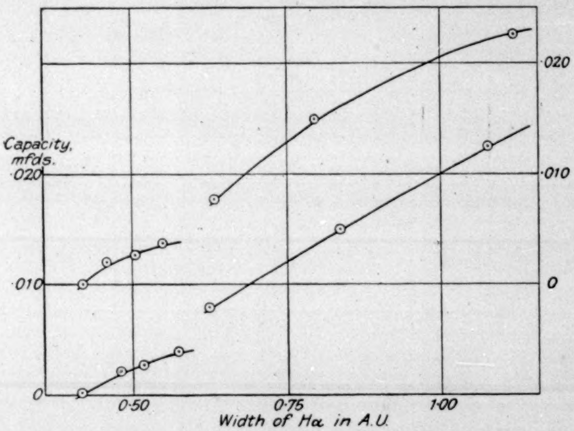

FIG. 2.

V. MAGNETIC CONTROL.

In the absence of a calculation of recombination of ions, the only other evidence which can be brought in favour of the dependence of broadening on the number of charged ions, instead of simply on average current density, appears to be the following: The concentration of charged ions in the neighbourhood of the radiating atoms must be varied, while the circuit controlling the current is kept constant. Now, the difference between a rectified and an unrectified discharge carrying the same current should supply this different concentration of ions.

It was found at once that a rectified discharge carrying the same current gave a narrower spectral line than when the main discharge was accompanied by a reverse current of more than half its magnitude. To investigate whether this difference held over a considerable range of widths, condensers could not be used, since their control would destroy the required constancy of period. The effect of a magnetic field was accordingly made use of. [I owe to Dr. Kapitza the suggestion of trying a magnetic control.]

The broadening of the line in a transverse magnetic field was measured for rectified and unrectified discharges (Fig. 3). In putting forward a possible explanation of these curves, three facts must be kept in mind :—

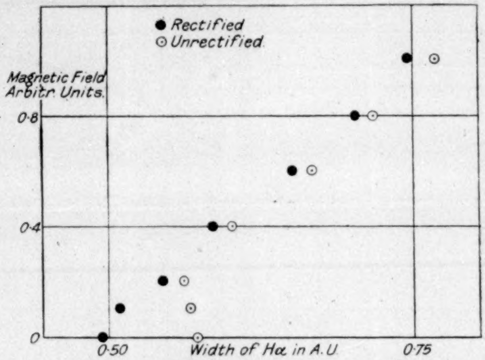

FIG. 3.

(*a*) The broadening is roughly proportional to the magnetic field.

(*b*) The broadening is always less for the rectified than for the unrectified discharge.

(*c*) The divergence between the rectified and unrectified curves is large at first, then decreases, but not to zero.

Consider how far the view we have been taking can explain these characteristics, the view, namely, that width depends on the number of charges which surround the radiating particle. The possible effects of the magnetic field are as follows :—

(i) Zeeman effect.

(ii) A crushing of the discharge against the wall of the tube, according to the deflection of a charged particle at right angles to its flight and to the field. This is known to produce (Kent and Frye : Astroph. J., April, 1913) an increase of the resistance of the tube, hence a decrease of the total current passing ; while the decrease of effective cross section of the discharge increases the current density.

(iii) The several other spectral effects of a magnetic field discussed by Kent and Frye, Waran, and others, e.g., the disintegration of the tube walls, can be neglected in these studies of the single line.

Let us apply the effects (i) and (ii) to the characteristics (*a*), (*b*), (*c*) of Fig. 3.

(*a*) The Zeeman effect, since it is linear with the field strength, will produce a broadening of the kind seen; before referring this characteristic solely to the Zeeman effect, however, the changes in current density must be carefully examined to see what the maximum disturbance due to effect (ii) could be.

For this purpose the following preliminary experiment was carried out.

To allow for the changes on rectifying, the resistance in the primary circuit of the induction coil had to be decreased slightly. Similar adjustments had to be made to allow for the distortion of the discharge in the magnetic field. These readjustments were in each case made until a milliammeter recorded the same current as was used throughout in the tube circuit. (A milliammeter is of no use in the capacity-inductance experiments, but can be used provided the period is unchanged.) To test whether any changes in breadth of line during the magnetic experiments could possibly be attributed to any of these alterations in the current through the tube, the discharge was varied over a range of 0·5 to 4·5 milliamps., by varying the primary current. The intensity of illumination increases enormously over this range, but the apparent line width only slightly, a result to be expected if the former is controlled by the number of radiating atoms, but the latter by their neighbouring charged particles. Now, the uncompensated milliammeter variations due both to the distortion of the discharge and also to rectification never amounted to more than a small fraction of the range of this preliminary experiment, and cannot therefore be held responsible for the broadening in the magnetic field, which is therefore attributed to the Zeeman effect.

(*b*) Consider next the experimental fact that unrectified broadening is always greater than that in a rectified discharge. On the view we have been taking of the importance of the concentration of ions, this fact appears natural; the unrectified discharge involves the presence of an additional stream of ions, whose charges will act on the atoms excited by the next impulse in the principal stream. This is the probable reason for the considerable difference between the rectified and unrectified line widths when the magnetic disturbance is zero.

(*c*) The mode of continuation of the curves when the magnetic disturbance is not zero seems, at first sight, strange; but consideration shows the anomaly may also be explicable on the same view of the charges surrounding the emitting particles.

Since positive and negative ions are moving in opposite directions, the rectified discharge is pressed against one wall of the tube, but in the unrectified discharge there is also another stream of positive and negative ions pressed against the opposite wall. Now, in the very intense fields used by Kent and Frye these two were quite separated into filaments clinging to the front and back walls of the tube. When this condition is reached, the charged particles in the reverse stream would be removed to what is virtually a separate path, and their influence on the emitting atoms of the direct stream would no longer be sufficient to increase widening. Rectified and unrectified discharges would then no longer differ in width. That, even at the strongest fields employed here, this condition is not reached is seen in the fact that the two curves never coalesce, though they are nearer than at the zero field. Since the fields used are far smaller than those of Kent and Frye, and since careful inspection of the tube never showed the separation of the two streams, the coalescence of the two graphs would not be expected. Some tendency to approach would, however, be expected as soon as the field is switched on, since it is only at zero field that the direct and reverse discharges completely fill the tube and interpenetrate; only in this condition will the maximum effect of the electric field of the charged particles in the reverse discharge be felt by the radiators, and the maximum difference between rectified and unrectified curves be seen. The wide separation of the curves at zero field, and the decreased separation as the magnetic field increases, are both to be expected from the known effects of a magnetic field, and support the view of the importance of the charges which surround the radiating atoms. Superposed on the Zeeman effect, which is common to both types of discharge, rectification and distortion in a magnetic field seem competent to explain the results seen in Fig. 3.

VI. THERMIONIC CONTROL.

The concentration of charges around the particles which emit the Balmer lines might also be altered by thermionic means; electrons, additional to those produced by the collision mechanism of the tube, might be drawn into the discharge without altering the circuit or the period.

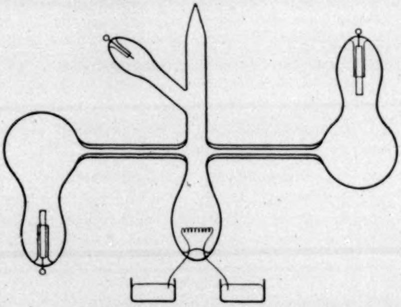

FIG. 4.

For this purpose a hydrogen tube was constructed with a branch which contained a hot filament. (Fig. 4.)

This tube had to be rebuilt more than once, and a platinum spiral wound round an asbestos fibre coated with lime was finally adopted as a source of thermions; this was heated by a battery insulated to avoid leakage of the main discharge.

Measurement of broadening was exceedingly difficult with this tube; in all cases the absorption of light in the echelon makes a very intense source of light necessary, but in this case the current had to be kept low if the number of added electrons was to be large enough in comparison with those already present in the main discharge, to make an appreciable difference to their concentration. The method is further limited by the occlusion and emission of gas by the filament at different temperatures, "glowing out" by the usual methods being inefficient when an atmosphere of hydrogen has to be retained for spectroscopic purposes.

In spite of these disadvantages, Fig. 5 shows distinctly that the line becomes narrower when the filament is lit up. The decrease in width, $\Delta\lambda$, is greatest for the smallest initial width; the several initial widths represent the several occasions on which a tube with the same filament and filament current was re-evacuated. The limitation mentioned above, that the original discharge must not produce so many ions that the thermionic source is inappreciable in comparison, is probably the reason for the tendency of $\Delta\lambda$ towards zero at higher initial widths, i.e., at higher pressures. This would account also for the failure to find a positive value of $\Delta\lambda$ even at higher pressures. On the theory we have found reasonable, decrease of line width means fewer free charges present; the added electrons from the filament, on this hypothesis, seem to act by recombination. This is to be expected, since their velocity will be small when they enter the main stream.

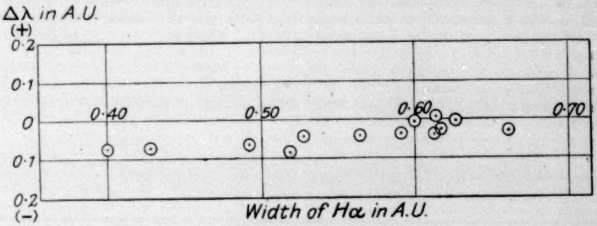

FIG. 5.

I would like to thank Prof. S. W. J. Smith, F.R.S., for the interest he has taken in this work, and for the ready facilities he has provided for carrying it out; and I am very grateful for the skill and patience of Mr. G. O. Harrison in constructing some of the apparatus required.

NOTE ADDED MAY 8, 1925.

The precise nature of the thermionic effect mentioned in §VI is not of the usual valve type; it has been pointed out to me by Dr. Kapitza that the insulation of heating battery will prevent any large constant stream of ions of one sign from leaving the filament. The question arises as to the amount of the evaporation of both positive and negative ions together. The differences of potential at the main electrodes will periodically facilitate this, and will draw both into the main discharge; if recombined in sufficient numbers, this will account for the narrowing seen in Fig. 5. It is hoped that experiments may be devised to throw light on whether these ions would be sufficient for the purpose.

DISCUSSION.

Mr. J. GUILD said that this interesting Paper contained some very suggestive results. The author had referred to Rossi's finding that a simple relation existed between the broadening of the lines and the current density, in the rather special sense in which the latter expression was used. Such a law should give a simple relation between the width of the lines and the intrinsic luminosity of the discharge. Had that point been tested ? Such a result seems unlikely in the case of heavy ions like those of mercury, because in such cases the width can be varied considerably, for a given intrinsic brightness, by varying the design of the tube. What effects would be observed where two or more elements were present in the discharge tube—in the case of a mixture of gases, for instance ? It might be expected, on the author's theory, that the width of a given line would in such cases be affected by the existence of neighbouring charged particles which were not themselves capable of emitting that line. The author had found that the range of his observations was limited by the overlapping of the various orders of the spectrum obtained with an echelon. Could he not, however, measure the broadening from the path difference over which interference would be visible ? The results so obtained would be of lower accuracy, but the range could be extended by this means.

Dr. L. SIMONS referred to Prof. Wien's work on the " duration of luminosity," or period of phosphorescence of atoms. A luminous atom can be conceived of in classical terms as emitting radiation with an amplitude which is exponentially damped, but the same phenomena can be equally well regarded in terms of the quantum theory as due to the statistical effect of a number of atoms each of which radiates a quantum catastrophically, the radiation being separated from the excitation by a period of time which varies from atom to atom in such a way as to produce a total effect identical with that of exponential damping. The author's results might be regarded from the classical point of view as due to the influence of ions on the decay factor of a damped oscillation, while on the quantum hypothesis the recognised theory of the Stark effect would be correspondingly applicable.

Dr. G. TEMPLE said that it had been found difficult to devise experimental tests for Sommerfeld's prediction of a resolution of $H\alpha$ into a pair of lines, each consisting of a triplet. Shrumm has recently succeeded in verifying this prediction, having photographed the fine structure of $H\alpha$, $H\beta$, and $H\gamma$, but he had to work at the temperature of liquid air. Would it be possible to study separately the widening of each component of such spectral lines ?

AUTHOR's reply : With regard to Mr. Guild's suggestion of using limits of interference, M. Procopin is working successfully on those lines for the relation of width to pressure. With regard to the case of elements of higher atomic number, the complexity of their Stark effect would make it more difficult to identify than in the simple symmetrical example of hydrogen. The only relation of width to intensity is that implied in the Merton-Nicholson equations I have quoted. Dr. Simons mentioned Wien's work ; that has certainly given the first clue to the time during which an atom remains excited. When the variation of this is understood, we may get a relation between ionisation and the broadening in a circuit of definite periodicity. Dr. Temple suggested the complication of the relativity structure of a line. For $H\alpha$ this covers at most 0·2 A.U., whereas my narrowest line was about 0·4 A.U. I am now working with a new tube at the effect on the fine structure.

XXII.—ON THE SPECTRA OF THE METALS OF THE ALUMINIUM SUB-GROUP.

By K. RANGADHAMA RAO, *M.A.*, Madras University Research Scholar.

Received March 16, 1925.

(*Communicated by* Dr. A. L. NARAYAN, *M.A.*, *D.Sc.*, *F.Inst.P.*)

ABSTRACT.

Continuing the previous work on absorption of light by thallium vapour, the author has now studied the absorption of Tl vapour from λ2400 to λ2000, and that of In from λ6000 to λ2000. The absorption tube was of steel, and provided with quartz windows at the ends, and absorption was studied with a quartz spectograph. Tables were given in each case of the wavelengths of the lines absorbed. The absorption spectra indicate marked similarities. The first members of the series, $1\pi_2—m\delta'$, of both Tl and In, are found to exhibit assymmetrical absorption. In both cases there appear at high temperatures and on the short wave side of $1\pi_2—2\delta'$ a channelled space spectrum consisting of assymmetrical bands degraded towards the red.

None of the lines of the principal series appeared in absorption, even at the highest temperatures used by the author. One remarkable feature is the very marked absorption of the members of $1\pi_2—m\delta'$.

INTRODUCTION.

THE study of the spectra of the metals of the aluminium sub-group is of particular interest for, in contrast with the series spectra of the alkali and the alkaline earth metals, $1\pi_2$ instead of 1σ is the highest observed spectral term for the aluminium metals. Further, these elements are trivalent, though the heavier metals of this group, particularly thallium, give stable mono-valent compounds. To account for these spectral and chemical properties, Bohr has recently made the assumption that, of the three outermost electrons in the atoms of these elements, two occupy normally 6_1 orbits, while the remaining one electron is more loosely bound in an orbit of azimuth number 2, and that the arc spectra of these metals give the virtual orbits of this single electron. Measurement of the radiation potentials, as well as a study of the absorption spectra of these metals is, therefore, of special importance, inasmuch as they indicate the orbit of the electron in the normal state of the atom, and test the validity of the above assignment of orbits of the electrons, put forward by Bohr.

Observations of the critical potentials and of the stages in the excitation of the arc spectra have been made on two occasions by Mohler* and others in the case of only one of these elements (thallium), and they find the ionization potential to be 6·08 volts, corresponding to the energy level $1\pi_2$, and lines of the sharp and diffuse series corresponding to jumps of the electron into the $1\pi_2$ orbit have been obtained as resonance lines at suitable excitation voltages.

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* F. L. Mohler and A. E. Ruark, Opt. Soc. Am. and Rev. Sci. Inst., Vol. 7, p. 819, October (1923).

It is well known that the arc spectra of these elements, especially thallium, present easy reversals of the subordinate series lines, which may be due to the fact that the atoms are in an excited state. A certain amount of work on the absorption of thallium vapour has been done during the last few years. In all these investigations the absorption of the vapour was studied only in the near ultra-violet, while in the case of In very little work is done, the only previous work on this being that of Grotrian.

Grotrian* examined the absorption spectra of the vapours of In and Tl up to $\lambda 2500$ nearly, and found that the lines of the sharp and diffuse series can be reversed, those corresponding to the electron jumps into the $1\pi_2$ orbit, however, appearing at lower temperatures.

Carroll† has also investigated the absorption of Tl vapour, extending his observations up to $\lambda 2500$.

In a recent Paper‡ on "Absorption and Dispersion of Thallium Vapour" an account has been given of experiments done by the author in collaboration with Prof. Narayan, indicating that the line $1\pi_2 - 1\pi_1$ which, being forbidden by the selection principle, failed to appear in Mohler and Ruark's experiments as a resonance line at the proper excitation potential of 0·9 volt, also remained unabsorbed by a column of non-luminous vapour of the metal. Experiments on dispersion showed that anomalous dispersion was more prominent at $\lambda 3775$ than at $\lambda 5350$, which is quite in conformity with the view that $1\pi_2$ is the ground orbit of the electron.

In the present investigation the study of the absorption of the non-luminous vapour of Tl has been extended lower down to $\lambda 2000$, and the same examination is made in the case of another unique element of this group—namely, In, from $\lambda 6000$ to $\lambda 2000$.

EXPERIMENTAL.

The method of experiment was the very simple one of passing light through a steel tube containing the vapour, provided with quartz windows, and examining the emergent light with a quartz spectrograph. This absorption tube of steel was exactly similar to the one used by the author previously, and described in detail elsewhere,§ and, to avoid any possible magnetic effect on absorption, it was heated in a charcoal furnace, through which a powerful blast of compressed air was maintained, the metal being enclosed in a vacuum. The temperature and density of the vapour were gradually raised, and the absorption spectra were photographed at different temperatures. Temperatures of the order of 1,500°C. could be easily obtained by this furnace, though, on account of the softening of the walls of the tube, it was not possible to keep the tube at this temperature for any length of time.

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* W. Grotrian, Zeits. f. Phy., Vol. 12, 3 and 4, pp. 218-31 (1922).

† Carroll, Proc. Roy. Soc., A, Vol. 103, pp. 334-339, May (1923).

‡ Narayan and others, Proc. Roy. Soc., A, Vol. 106, pp. 596-601 (1924).

§ Phil. Mag., May (1923).

In many of these experiments the continuous background furnished by a high-frequency spark under water, which passed between two stout aluminium electrodes, was used as the source of light. The distance between the electrodes could be adjusted by a micrometer screw. Fig. 1 shows the electrical connections for this spark. Exposures ranging from 1 to 20 mins. were given, depending on the spectral region to be examined. In the case of both metals, Schumann plates supplied by Adam Hilger, Ltd., were used to photograph the region λ2400-λ2000.

THALLIUM.

As has been mentioned above, the absorption spectrum of this metal has been previously photographed by the author up to λ2400, so that in the following table only the additional lines, since photographed in the region λ2400-λ2000, are included:

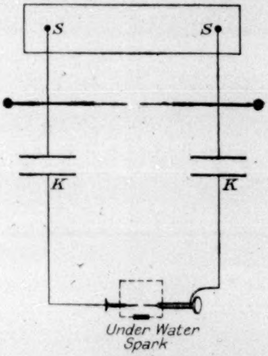

FIG. 1.

Wavelength of Line.	Designation.	Remarks.
2315	$1\pi_2—3\sigma$	
2207	$1\pi_2—4\sigma$	
2152	$1\pi_2—5\sigma$	
2119	$1\pi_2—6\sigma$	
2098	$1\pi_2—7\sigma$	
2379	$1\pi_2—3\delta'$	
2237	$1\pi_2—4\delta'$	
2168	$1\pi_2—5\delta'$	
2129	$1\pi_2—6\delta'$	
2105	$1\pi_2—7\delta'$	
2136	......	**These lines do not fit into any series system.**
2203	......	
2532	......	

The accompanying diagram (Fig. 2) of the scheme of energy levels brings out clearly the orbital transitions involved in the absorption of the above members of the series, and shows that these lines result from the transitions to the lowest energy level, $1\pi_2$.

Plate A is a reproduction of the absorption spectra obtained.

Strip (*a*) is the absorption spectrum from about λ2400—λ2230 A.U.
(*b*) ,, ,, ,, λ2190—λ2090 ,,
*(*c*) ,, ,, ,, λ2400—λ2230 ,,
*(*d*) ,, ,, ,, λ2220—λ2115 ,,

* Portions of the above region taken at higher temperatures.

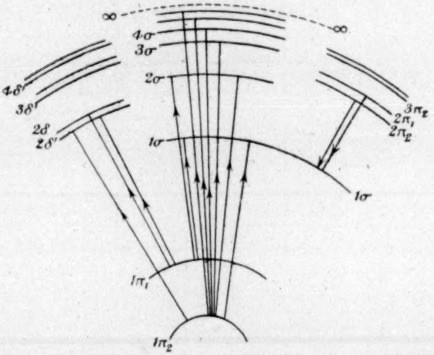

FIG. 2.—ENERGY DIAGRAM OF THE SERIES SYSTEMS OF THALLIUM.

From an examination of the above spectra it is easily seen that at each of the lines λλ2379, 2231, etc.—i.e., lines of the diffuse series corresponding to the electron jumps from the $1\pi_2$ orbit—the absorption is very diffuse and complex; almost every line seems to be accompanied by close satellites, which fuse together and merge into the main line, forming a broad band at high temperatures. This feature is most obvious from the lines λλ2379, 2237 [Plate A], which are distinctly seen to be followed by close satellites which, with the increase of vapour density, merge into the main component to form a broad band.

The absorption is much more intense than in the case of the lines of the sharp series.

The assymmetrical development already observed very well in the case of λλ3775 and 2768 is noticed, as is evident from Plate A (*c*), also in the case of λλ2379, 2315.

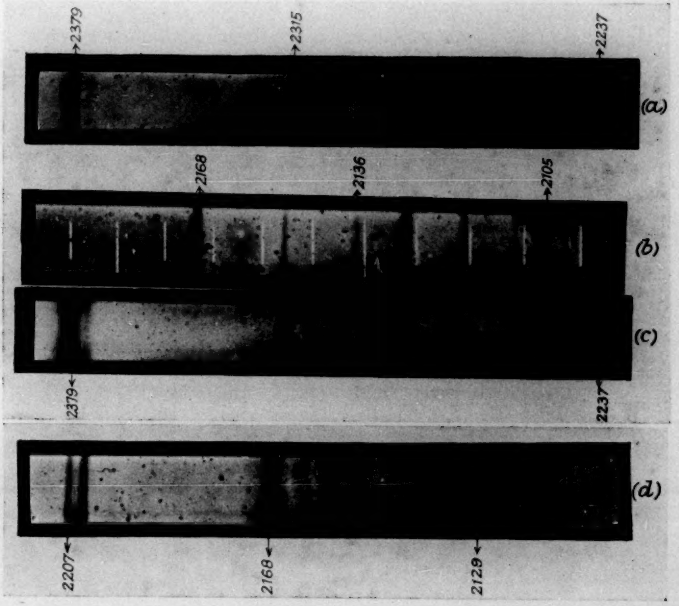
2379
2315
2237
(a)
2168
2136
2105
(b)
(c)
2379
2237
(d)
2207
2168
2129

PLATE A.—Tl

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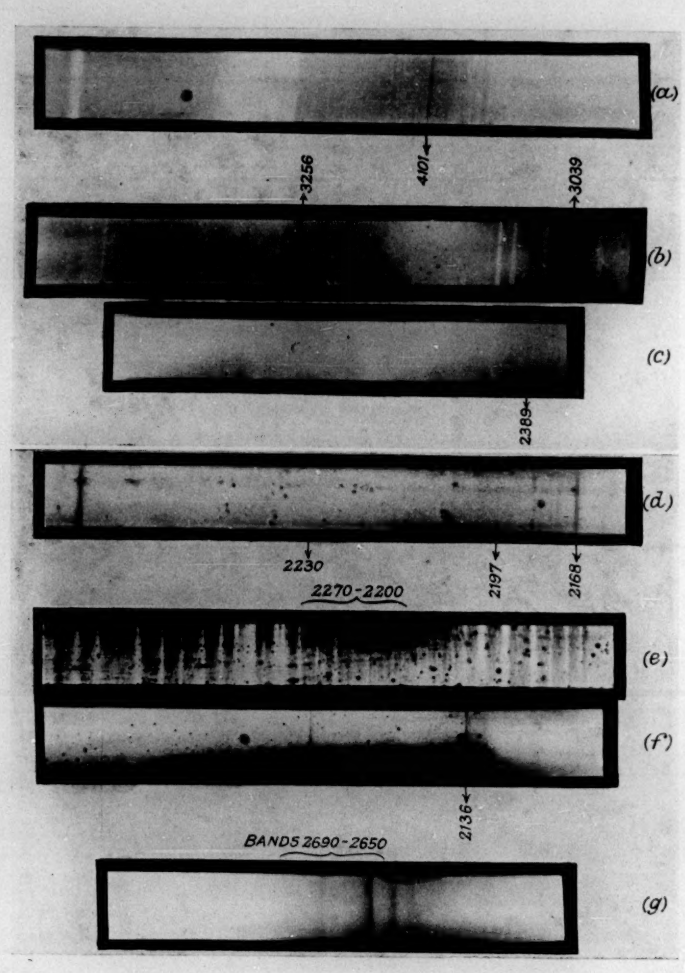
(a)
3256
4101
3039
(b)
(c)
2389
(d)
2230
2197
2168
2270-2200
(e)
(f)
2136
BANDS 2690-2650
(g)

PLATE B.—In.

[To face page 263.

INDIUM.

In the table below is given a list of the absorption lines that have been photographed with Indium vapour, and Fig. 3 represents the scheme of energy levels constructed for indium, just as in the case of thallium.

Wavelength of Line.	Designation.	Remarks.
4501	$1\pi_1 - 1\sigma$	
4101	$1\pi_2 - 1\sigma$	
2932	$1\pi_1 - 2\sigma$	
2753	$1\pi_2 - 2\sigma$	
2460	$1\pi_2 - 3\sigma$	
3258	$1\pi_1 - 2\delta'$	
56	$1\pi_1 - 2\delta$	
3039	$1\pi_2 - 2\delta'$	
2713	$1\pi_1 - 3\delta'$	
10	$1\pi_1 - 3\delta$	
2560	$1\pi_2 - 3\delta'$	
2522	$1\pi_1 - 4\delta'$	
21	$1\pi_1 - 4\delta$	
2389	$1\pi_2 - 4\delta'$	
2306	$1\pi_2 - 5\delta'$	
2260	$1\pi_2 - 6\delta'$	
2230	$1\pi_2 - 7\delta'$	
2211	$1\pi_2 - 8\delta'$	
2197	$1\pi_2 - 9\delta'$	
2187	$1\pi_2 - 10\delta'$	
2179	$1\pi_2 - 11\delta'$	
2290	......	These lines do not fit into any series system.
2168	......	
2136	......	

Besides these, we have a group of very fine lines at λ2200-2270, as shown in Plate B (*e*).

The chief difficulty experienced in these experiments on the absorption of indium is that, at temperatures as low as about 1,000°C., the vapour exhibits general continuous absorption beyond λ3020 (a fact which does not seem to have been observed by Grotrian), which prevents the photographing of the members of the line series in their developed stage.

The absorption spectra of In and Tl present marked similarities.

As in Tl no trace of absorption lines corresponding to the principal series is detected, but only lines of the sharp and diffuse series were obtained with ease and reproduced in Plate B.

It is of great interest to observe that in the case of both the metals the absorption in $1\pi_1$ series is very feeble, while it is more marked in the lines of $1\pi_2$ series, especially lines of $1\pi_2 - m\delta'$. This marked absorption in the series $1\pi_2 - m\delta'$ probably indicates that the transitions of the electron between $1\pi\pi_2$ and δ' orbits is more easy, and consequently more frequent.

The absorption band at λ3039 ($1\pi_2 - 2\delta'$), like the corresponding line λ2768 of Tl, is distinctly assymmetrical, being degraded towards the long wavelength side. In another feature, too, the absorption spectrum of indium proved similar to that of thallium—namely, in exhibiting at high vapour densities and on the short wavelength side of $1\pi_2 - 2\delta'$, a typical banded spectrum which, in both cases, consists of clearly asymmetrical bands shaded off towards the red end. This banded spectrum (in the case of In) extending over the region 2650-2690 is well brought out in spectrogram *g*, Plate B.

At the same time, it has been found that there is one remarkable difference between the absorption of In and that of Tl, inasmuch as the absorption of Indium is very feeble when compared with that of Tl, though the melting and boiling points of In are distinctly lower than those of thallium. This marked difference between the absorption spectra may possibly be due to the difference in their valencies. From the chemistry of these metals we know that univalent thallium forms a well-defined series of stable salts, while compounds of trivalent thallium are ill-defined and unstable, and those of trivalent In are strong and stable. On the other hand, aluminium is predominantly trivalent. In this and in other ways thallium differs from other members of this sub-group. This strong trivalent nature of In and Al, and the predominantly monovalent nature of Tl, possibly suggests that in the former metals the arc spectrum arises from interorbital transitions of one electron of a group of three, while the arc spectrum of thallium arises in the orbitral transitions of the electron having azimuth number two.

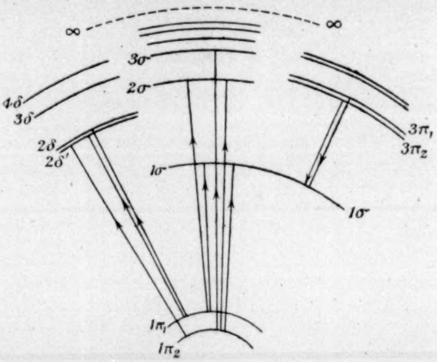

Fig. 3.—Energy Diagram of the Series System of Indium.

However, an account of the great similarity existing between the spectral and chemical properties of the elements of this sub-group, we can tentatively predict the critical potentials of Al, Ga, and In from their known spectral terms, and the following table gives the values of the critical potentials thus calculated for the elements of this sub-group.

Element.	$1\pi_2$ term.	Ionization Potential computed from Quantum Relation.	Excitation Potentials (Volts.)		
			$1\pi_2-1\pi_1$	$1\pi_2-2\delta'$	$1\pi_2-1\sigma$
	Cm.$^{-1}$	Volts.			
Al ...	48,281	5·96	0·014	4·002	3·13
Ga ...	48,380	5·97	0·102	4·29	3·06
In ...	46,668	5·76	0·27	4·06	3·01

In conclusion, I desire to express my great indebtedness to Prof. A. L. Narayan for much valuable help during the progress of the work.

DEMONSTRATIONS EMPLOYING THE CATHODE-RAY OSCILLOGRAPH.

Shown at the Meeting on March 27th, 1925.

By Mr. Norman Kipping, of the International Western Electric Company.

I. The Use of the Neon Lamp for Producing a Uniform Time Base.

It is well known that a neon lamp when shunted by a condenser, the combination being in series with a resistance and battery, passes current intermittently at a definite frequency; the condenser is charged by the current through the resistance until the voltage across it reaches the upper critical voltage of the lamp, whereupon the condenser discharges through the lamp until the lower critical voltage is reached. In the ordinary arrangement the potential drop across the resistance varies exponentially with time, but according to the present system the resistance is replaced by a diode valve which under the conditions of the experiment is run at saturation, so that the charging current to the condenser is constant. The potential drop across the condenser, and consequently that across the diode, then varies linearly with time, and the latter is used for the production of a uniform time base. The frequency is preferably controlled by varying the capacity of the condenser, but alternatively the filament-heating current of the diode may be varied. It is essential that the current through the diode be the saturation current of the latter throughout the charging of the condenser. Wave forms up to a frequency of 50,000 ∿ were plotted with this apparatus.

II. An Examination of Some Dynamic Characteristics of Valves.

A valve oscillator with reaction coupling is connected to the oscillograph, so that anode current is plotted against grid voltage, the amplitude of the oscillations being controlled by means of a damping resistance. When two sinusoidal quantities which are in phase are plotted against one another the graph is linear. In practice, however, there is a phase difference between the two quantities mentioned above, owing to the fact that the inductances and capacities employed are not pure, and consequently the oscillograph trace is elliptical in form so long as the straight part of the valve characteristic is adhered to. As the amplitude of the oscillations increases, however, distortion sets in owing to the following causes: (*a*) At the lower values of the grid potential the current approaches zero non-linearly. (*b*) At the higher values of the current, the latter approaches saturation. (*c*) The anode potential varies sinusoidally out of phase with the grid potential, and may even drop to zero for part of the cycle, so that the current in this case becomes zero twice per cycle. In consequence of phase lag the oscillograph trace becomes boot-shaped. Figures varying between the ellipse and the boot were shown, and each of the variables was plotted separately against time. It is found that the maximum obtainable plate current in an oscillating valve is limited not so much by the occurrence of grid current as by the drop in plate potential.

III. DETERMINATION OF PERCENTAGE MODULATION AND OF DISTORTION IN VALVE TRANSMITTERS.

Percentage modulation is the quantity $\frac{a-b}{a}\times 100$, where a is the maximum and b the minimum amplitude of the carrier wave. This can conveniently be measured by plotting the modulated output current from a valve against the audio-frequency or modulating input voltage. If the modulation is complete (i.e., 100 per cent.) and distortionless the resulting trace will comprise an isosceles triangle whose base is proportional to a and whose height is proportional to the amplitude of the audio-frequency input voltage. If modulation is not complete, the triangle becomes a trapezium of which the longer base is proportional to a and the shorter base to b. Further, there is normally a phase difference between the audio-frequency input and the envelope of the modulated carrier-wave, so that the sloping sides of the trapezium may become ellipses; this does not, however, invalidate the determination of percentage modulation by this method. Over-modulation is shown by the sloping sides of the figure meeting at a point which is not at their ends, and radio-frequency distortion is shown by lack of symmetry in the figure. If the carrier-wave contains harmonics, a series of superposed triangular figures is obtained. In the demonstration the above features were shown at a carrier frequency of about 50,000 ∿, and the carrier-wave was also plotted against time both when modulated and when unmodulated, the audio-frequency component of the output being removed by means of a high-pass filter with a cut-off at 3,000 ∿. It was shown that when the modulation was 60 per cent. for a sinusoidal audio-frequency wave-form, over modulation at the apex could be produced by adding harmonics to this wave-form.

DEMONSTRATION ON THE DIFFRACTION OF LIGHT BY A SPHERICAL OBSTACLE.

Shown at the Meeting on May 22nd, 1925.

By Prof. A. O. Rankine, O.B.E., D.Sc.

IT was pointed out by Prof. Porter in 1914 that, owing to the existence of the well-known white spot at the centre of a circular shadow, a circular obstacle could be used to project an image of an extended source of light. Each point in the source of light yields its own white spot, and the sum total of the white spots constitutes an inverted image which obeys the same law of magnification as the image due to a pinhole. Owing to the fact that only one point in the source of light can lie on the optical axis a certain amount of aberration occurs with a circular obstacle, but Prof. Rankine avoids this difficulty by using a spherical obstacle, such as a steel bearing-ball. Sources of light of various shapes and about 2 mm. in linear dimensions were made by pasting black strips on a lantern slide ; a ball of about 7 mm. diameter was fixed a few feet away from this, and the image, having a magnification of about 3, was exhibited on a ground glass screen.

XXIII.—ON MASS AND ENERGY.

By G. TEMPLE, *Ph.D.*, Imperial College of Science.

Received April **21, 1925.**

ABSTRACT.

It is assumed that variations in the potential energy of a body (gravitational or electrostatic) are always accompanied by proportionate changes in its mass. Combining this assumption with the theories of Newtonian dynamics and Maxwellian electrodynamics, it has been found possible to predict all those phenomena, which are usually regarded as the crucial tests of the theories of relativity, both " special " and " general."

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§1. Introduction.
§2. The Equivalence of Mass and Energy.
§3. The Secular Advance of the Perihelion of Mercury.
§4. The Deviation of Light-Rays by the Sun.
§5. The Displacement of the Fraunhöfer Lines.
§6. The Variation of Electronic Mass with Velocity.
§7. The Fine-Structure of the Spectrum of H and He^+.
§8. Conclusion.

§1. INTRODUCTION.

REGARDED from the standpoint of the experimental physicist, the justification of a theory of relativity is to be found in the fact that it satisfies both of the following conditions. In the first place, to a high degree of approximation, the relativistic theory must reproduce the essential features of classical dynamics and electromagnetism, which are, of course, substantially accurate summaries of experimental facts. In the second place, the slight corrections and modifications made by the relativistic theory must be such as to afford some explanation of those phenomena which have eluded investigation by the older theories. In this Paper we shall discuss a number of phenomena, the explanation of which has been one of the triumphs of the General and Special Theories of Relativity, basing our discussion on an hypothesis which seems to be more congenial to physicists than the elusive metaphysical basis of the theories of Einstein and Whitehead. It will transpire that the simple assumption we shall make, supported by elementary mathematics, is sufficient to account for most of the " crucial " phenomena of relativity.

§2. THE EQUIVALENCE OF MASS AND ENERGY.

In making a slight modification of the classical theories of physics it is an obvious advantage to conserve as far as possible the mathematical form of these theories. To this end we shall adopt in our new theory all the fundamental laws of Newtonian dynamics, making but one alteration—we shall not assume the masses of our moving bodies to be constants, but shall assume them to vary with the potential energies of the bodies, in a fashion to be made precise in the following manner.

Let m denote the mass of a particle, and K denote its potential energy, while c denotes the velocity of light in vacuo and t the time. Then our fundamental assumption is expressed by the equation

$$\frac{d}{dt}\left(\frac{1}{2}mc^2\right)+\lambda\frac{dK}{dt}=0 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}1)$$

where λ equals unity when the energy is gravitational, and equals one-half when the energy is electrostatic. As the potential energy of a particle decreases, there will be a proportionate increase in its mass. The first term in the above equation has a form which recalls an hypothesis of Sir J. J. Thomson(1), according to which all potential energy is kinetic in origin.

Let U denote the potential of a gravitational field of force at any point, and m denote the mass of a particle when placed at the same point. Then

$$K=mU \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}2)$$

and

$$\frac{d}{dt}\left(\frac{1}{2}mc^2\right)+\frac{d}{dt}\,(mU)=0 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}3)$$

from equations (2·1) and 2·2).

Integrate this equation from $t=t_1$ to $t=t_2$, and let m_1, m_2, U_1, U_2 denote the initial and final masses and potentials respectively. Then

$$\tfrac{1}{2}\,(m_2-m_1)\,c^2=-(m_2U_2-m_1U_1)$$

or

$$m_2=\frac{1+\dfrac{2U_1}{c^2}}{1+\dfrac{2U_2}{c^2}}\,.\,m_1 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}4)$$

If m_0 denotes the mass of the particle when removed to an infinite distance from the matter producing the field of force, and if we adopt the usual convention that U vanishes at infinity, then we obtain the results

$$m=\frac{m_0}{1+\dfrac{2U}{c^2}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}5)$$

and

$$mU=\tfrac{1}{2}m_0c^2-\tfrac{1}{2}mc^2 \quad . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}51)$$

If x, y, z denote the Cartesian co-ordinates of the particle at time t, the Newtonian equations of motion will be

$$\frac{d}{dt}\left\{m\frac{dx}{dt},\ m\frac{dy}{dt},\ m\frac{dz}{dt}\right\}=-\frac{\partial}{\partial x,\,y,\,z}\,(mU) \quad . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}6)$$

We shall transform these equations by taking as our new independent variable the "proper time" τ defined by the integral

$$\tau=\int_0\left(1+\frac{2U}{c^2}\right)dt \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2{\cdot}7)$$

taken along the path traversed by the particle from the time $t=0$. Noticing that

$$\frac{dt}{d\tau}=\left(1+\frac{2U}{c^2}\right)^{-1}=\frac{m}{m_0}\qquad\ldots\ldots\ldots\ldots\qquad(2\cdot71)$$

we obtain the result

$$m_0\frac{d^2}{d\tau^2}\cdot\{x, y, z\}=-\frac{m}{m_0}\,\frac{\partial}{\partial x, y, z}\,(mU)$$

$$=\frac{c^2}{4m_0}\cdot\frac{\partial}{\partial x, y, z}\,(m^2)\text{ from }(2\cdot51)$$

$$=-\frac{m_0\,.\,\partial V}{\partial x, \partial y, \partial z}\qquad\ldots\ldots\ldots\ldots\qquad(2\cdot8)$$

where $$V=-\frac{c^2}{4}\left(1+\frac{2U}{c^2}\right)^{-2}\sim-\frac{c^2}{4}+U-\ldots\ldots\ldots\ldots\qquad(2\cdot81)$$

from (2·71).

Hence the path described by a particle is exactly that which would have been obtained on the classical theory in a field of force of potential V.

§3. THE SECULAR ADVANCE OF THE PERIHELION OF MERCURY.

An immediate deduction from the conclusion of the preceding paragraph is that the equations to the trajectory of a particle used in Newtonian dynamics may also be utilised in our new theory, provided we take as the potential the function V. For a central field of force due to a massive immovable body of mass M, the true potential is given by the equation

$$U=-\frac{\gamma M}{r}\qquad\ldots\ldots\ldots\ldots\qquad(3\cdot1)$$

where γ is the constant of gravitation ($=6\cdot66\times10^{-8}$ gms.$^{-1}$. cms.^{3}. secs.$^{-2}$), and r is the radius vector drawn from the centre of the attracting body. Hence, using u to denote the reciprocal of r, we find from equation (2·81) that the "apparent" potential is

$$V=-\frac{c^2}{4}\,.\,(1-2\mu\,.\,u)^{-2}\qquad\ldots\ldots\ldots\ldots\qquad(3\cdot11)$$

where $\mu=\frac{\gamma M}{c^2}=$ the "gravitational radius" of the attracting body. Taking the mass of the sun as $M=1\cdot99\times10^{33}$ grams, we find that its gravitational radius is $\mu=1\cdot47$ kilometres.

The differential equation to the orbit of a planet may now be written in the familiar form

$$\frac{d^2u}{d\theta^2}+u=\frac{P}{h^2u^2}\qquad\ldots\ldots\ldots\ldots\qquad(3\cdot2)$$

where θ is the angular co-ordinate, h is Kepler's constant equal to $r^2\dfrac{d\theta}{d\tau}$, and P the apparent central acceleration is equal to

$$\frac{\partial V}{\partial r}=\frac{\mu c^2u^2}{(1-2\mu u)^3}$$

Hence equation (3·2) may be written

$$\frac{d^2u}{d\theta^2}+u=\frac{\mu c^2}{h^2(1-2\mu u)^3} \tag{3·21}$$

which differs from the corresponding equation of the classical theory, in which the term $(1-2\mu u)^3$ is replaced by unity.

A first integral of this equation is

$$\left(\frac{du}{d\theta}\right)^2+u^2=\frac{c^2}{2h^2(1-2\mu u)^2}+\text{const.} \tag{3·22}$$

whence it appears that the complete integral will involve elliptic functions.

Since the radius vector of the orbit of the planet Mercury is of the order of $5{\cdot}77\times10^7$ *Km*, the magnitude of the term $2\mu u$ is about $0{\cdot}5\times10^{-7}$, and in expanding $(1-2\mu u)^{-3}$ by the binomial theorem to obtain an approximate answer we may neglect higher powers of $2\mu u$ than the first. We thus obtain the equation

$$\frac{d^2u}{d\theta^2}+u=\frac{\mu c^2}{h^2}\,.\,(1+6\mu u)$$

or

$$\frac{d^2u}{d\theta^2}+\kappa^2u=\frac{\mu c^2}{h^2} \tag{3·3}$$

where

$$\kappa^2=1-6\frac{\mu^2c^2}{h^2} \tag{3·31}$$

Hence the equation to the orbit is

$$l\,.\,u=1+\varepsilon\cos\kappa\theta \tag{3·4}$$

where

$$\kappa^2h^2=l\,.\,\mu\,.\,c^2 \tag{3·41}$$

This represents an ellipse, of eccentricity ε and semi-latus-rectum l, the major axis of which slowly revolves about a focus. If a is the semi-axis-major of the orbit,

$$l=a(1-\varepsilon^2) \tag{3·5}$$

Hence the advance of the perihelion in radians per revolution is

$$\frac{2\pi}{\kappa}-2\pi=\frac{6\pi^2\mu c^2}{h^2}$$

$$=\frac{6\pi\gamma M}{c^2a(1-\varepsilon^2)}\quad\text{From (3·41) and (3·5)} \tag{3·6}$$

the same result as that obtained on the theories of Einstein and Whitehead—namely, 42·9 seconds of arc per century.

§4. THE DEVIATION OF LIGHT-RAYS BY THE SOLAR FIELD.

It may be recalled that on relativistic theories the action of a gravitational field on the propagation of light is introduced in the following way. The paths of material particles are obtained as the geodesics of a manifold whose line element has the form

$$ds^2 = \sum_{1}^{4}{}^{(\mu\nu)} g_{\mu\nu} dx^\mu dx^\nu \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (4{\cdot}1)$$

and it is then assumed that the tracks of rays of light may be obtained as the minimal lines of the same manifold. In order to exhibit the effect of a gravitational field according to the theory now being developed, I adopt an hypothesis which seems no more extravagant than that just mentioned.

It appears from equations (2·8) and (2·81) that the paths of material particles are the geodesics of the manifold whose line element is

$$ds^2 = (W - V)(dx^2 + dy^2 + dz^2) \quad . \; . \; . \; . \; . \; . \quad (4{\cdot}2)$$

where W is a constant equal to $\frac{1}{2}\left\{\left(\frac{dx}{d\tau}\right)^2 + \left(\frac{dy}{d\tau}\right)^2 + \left(\frac{dz}{d\tau}\right)^2\right\} + V$, and determined by the circumstances of the projection of the particle. (*ds* is, of course, proportional to the element of action of the particle.)

By writing $W=0$ we obtain a form which is particularly simple, and free from any reference to the velocity of projection of a material particle. In this case we find that

$$ds^2 = \frac{c^2}{4}\left(1 + \frac{2U}{c^2}\right)^{-2} (dx^2 + dy^2 + dz^2) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (4{\cdot}3)$$

We shall assume that the equations of the electromagnetic field are covariant for all transformations which leave ds^2 invariant. These equations could easily be formed as suggested by Ricci and Levi—Civita (2); but it is sufficient for our purpose to note that Fermat's principle can now be stated in the form that the paths of light rays render the integral

$$\int \frac{\sqrt{dx^2 + dy^2 + dz^2}}{c\left(1 + \frac{2U}{c^2}\right)}$$

a minimum, the velocity of light being

$$c\left(1 + \frac{2U}{c^2}\right) = c(1 - 2\mu \,.\, u)$$

for the gravitational field of the sun.

This yields, as in the theory of relativity, the result $\frac{4\mu}{R}$ for the deviation in radians of a ray of light passing near the sun, the minimum distance of the ray from the centre of the sun being R.

§5. THE DISPLACEMENT OF THE FRAUNHÖFER LINES IN THE SOLAR SPECTRUM.

If we endeavour to imitate the naïve procedure of those relativists who assume that atomic clocks beat out equal values of the ds of equation (4·1) wherever situated, we are led to assume that two similar atoms placed at different points register equal intervals of the proper time τ, so that their true periods of vibration ν_1 and ν_2 are in in the ratio

$$\frac{\nu_1}{\nu_2}=\frac{1-2\mu\,.\,u_2}{1-2\mu\,.\,u_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5{\cdot}1)$$

when situated in the solar field at distances u_1 and u_2 from the sun. This leads to a shift of the solar lines towards the infra-red of an amount

$$\triangle\nu=\left(\frac{2\mu}{R}\right).\,\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5{\cdot}2)$$

R being the radius of the sun.

This is twice the amount of the Einstein-shift, and although the experimental evidence is at present inconclusive, the existence of the shift given above seems less likely than the existence of the Einstein-shift.

§6. THE VARIATION OF ELECTRONIC MOMENTUM WITH VELOCITY.

In forming the equations of motion of an electron, the only modification which we shall make of classical electrodynamics will be that the mass of the electron varies with its electrostatic energy as described by equation (2·1). Let m and e denote the electronic mass and charge (in electrostatic c.g.s. units), and let E_x, E_y, E_z, H_x, H_y, H_z denote the components of the electric and magnetic intensities in electrostatic and electromagnetic units respectively. Then the equations of motion are

$$\frac{d}{dt}\left(m\frac{dx}{dt}\right)=e\left\{E_x+\frac{1}{c}\left(\frac{dy}{dt}\,.\,H_z-\frac{dz}{dt}H_y\right)\right\}, \text{ \&c.} \quad . \quad . \quad . \quad . \quad . \qquad (6{\cdot}1)$$

As in paragraph 2, we transform these equations by taking as our new independent variable the " proper time " τ, defined by the integral

$$\tau=\int_0 \frac{m_0\,.\,dt}{m} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6{\cdot}2)$$

taken along the path of the electron from the position occupied at $t=0$. We then find equations (6·1) become

$$m_0\frac{d^2x}{d\tau^2}=e\left\{\frac{m}{m_0}\,.\,E_x+\frac{1}{c}\left(\frac{dy}{d\tau}\,.\,H_z-\frac{dz}{d\tau}H_y\right)\right\} \quad . \quad . \quad . \quad . \quad . \qquad (6{\cdot}3)$$

From these equations we deduce the energy-integral

$$\frac{m_0}{2}\left\{\left(\frac{dx}{d\tau}\right)^2+\left(\frac{dy}{d\tau}\right)^2+\left(\frac{dz}{d\tau}\right)^2\right\}=\frac{e}{m_0}\int m(E_x\,.\,dx+E_ydy+E_zdz)+W. \quad . \quad . \qquad (6{\cdot}31)$$

where W is a constant equal to the total energy.

Let ψ denote the electrostatic potential, so that

$$E_x=-\frac{\partial\psi}{\partial x},\quad E_y=-\frac{\partial\psi}{\partial y},\quad E_z=-\frac{\partial\psi}{\partial z} \qquad \text{(6·4)}$$

while the electrostatic energy of the electron is

$$K=e\psi \qquad \text{(6·41)}$$

Then from equation (2·1)

$$m=m_0-\frac{e\psi}{c^2} \qquad \text{(6·42)}$$

provided that m_0 denotes the mass of the electron in a position where ψ vanishes. Since equations (6·4) and (6·42) yield the result

$$cmE_x=\frac{1}{2}c^2\frac{\partial}{\partial x}(m^2),\ \text{\&c.} \qquad \text{(6·5)}$$

we obtain from this last equation and from equation (6·31) the relation

$$\frac{1}{2}m_0\left\{\left(\frac{dx}{d\tau}\right)^2+\left(\frac{dy}{d\tau}\right)^2+\left(\frac{dz}{d\tau}\right)^2\right\}=\frac{c^2m^2}{2m_0}+W \qquad \text{(6·51)}$$

Let the electron be initially at rest at a point where the electrostatic potential is zero. Then, from equation (6·51) we have that

$$W=-\tfrac{1}{2}m_0c^2 \qquad \text{(6·52)}$$

and

$$\frac{1}{2}m_0\left\{\left(\frac{dx}{d\tau}\right)^2+\left(\frac{dy}{d\tau}\right)^2+\left(\frac{dz}{d\tau}\right)^2\right\}=\frac{c^2}{2m_0}(m^2-m_0^2) \qquad \text{(6·53)}$$

Multiply the last equation by $\left(\frac{d\tau}{dt}\right)^2$, which by equation (6·2) equals $\left(\frac{m_0}{m}\right)^2$, and we obtain

$$\tfrac{1}{2}m_0v^2=\tfrac{1}{2}m_0c^2-\tfrac{1}{2}m_0^3.\ m^{-2}c^2$$

where v denotes the resultant velocity of the electron.

Rewriting the last equation, we obtain the familiar result

$$m=\frac{m_0}{\sqrt{1-\frac{v^2}{c^2}}} \qquad \text{(6·54)}$$

which agrees with the Lorentz' formula for the variation of the momentum of an electron with its velocity.

Hence the theory developed in this Paper will interpret the experiments of Kaufman and Bucherer, and similar investigations which study the trajectories of electrons in electromagnetic fields, and which are usually explained by means of the Special Theory of Relativity.

§7. THE FINE STRUCTURE OF THE SPECTRUM OF He^+ AND H.

There is another class of phenomena, which depend for their elucidation on the variability of electronic mass, but which are not completely covered by the researches of the last paragraph. These are the phenomena of the fine structure of the spectra of hydrogen and of ionised helium. In the special theory of relativity the kinetic energy of an electron moving with velocity v is taken as

$$m_0c^2\left\{\frac{1}{\sqrt{1-\frac{v^2}{c^2}}}-1\right\}$$

while the left-hand side of equation (6·52) above is equal to

$$\frac{\frac{1}{2}m_0v^2}{1-\frac{v^2}{c^2}}$$

an expression which plays the part of kinetic energy of the electron in our new theory.

Since the Bohr-Sommerfeld theory of the hydrogen spectrum involves the first expression for the energy, it is necessary for us to show in detail that the same predictions may be made on the basis of the present theory.

It is simpler to abandon the special convention of equations (6·52), (6·53) and (6·54) and to utilise equations (6·3) and (6·51). The potential at any point due to a fixed positive nucleus carrying a charge E (electrostatic units) is E/r, where r is the distance of the point considered from the nucelus. Hence, if m_0 denotes the mass of the electron when removed to an infinite distance, equation (6·42) becomes

$$m=m_0-\frac{eE}{c^2r} \quad \text{. . . . .} \quad (7{\cdot}1)$$

while equations (6·3) may be written as

$$\frac{d^2x}{d\tau^2}=-\frac{\partial V}{\partial x}, \text{ \&c.} \quad \text{. . . . .} \quad (7{\cdot}2)$$

where $$V=-\frac{c^2}{2}\cdot\left\{1-\frac{eE}{m_0c^2r}\right\}^2=-\frac{c^2}{2}+\frac{eE}{m_0r}-\frac{e^2E^2}{2m_0{}^2c^2r^2} \quad \text{. . . . . .} \quad (7{\cdot}21)$$

It follows that the orbit of the electron has the same form as if a particle of unit mass were moving under the influence of a field of force of potential V in Newtonian dynamics. The equation to the orbit is therefore

$$\frac{d^2u}{d\theta^2}+u=\frac{P}{h^2u^2} \quad \text{. . . . .} \quad (7{\cdot}3)$$

where the central acceleration

$$P=\frac{\partial V}{\partial r}=-\frac{eE}{m_0r^2}+\frac{e^2E^2}{m_0{}^2c^2r^3} \quad \text{. . . . .} \quad (7{\cdot}31)$$

and $$r^2\frac{d\theta}{d\tau}=h \quad \text{. . . . .} \quad (7{\cdot}32)$$

Hence equation (7·3) becomes

$$\frac{d^2u}{d\theta^2}+\kappa^2u=-\frac{eE}{m_0h^2} \quad \text{. . . . .} \quad (7{\cdot}33)$$

where $$\kappa^2=1-\left(\frac{eE}{m_0ch}\right)^2 \quad \text{. . . . .} \quad (7{\cdot}34)$$

The equation to the orbit is therefore

$$l\,.\,u=1+\varepsilon\cos\kappa\theta \quad \ldots\ldots\ldots \quad (7{\cdot}35)$$

where

$$\kappa^2=-\frac{eE}{m_0h^2}\,.\,l \quad \ldots\ldots\ldots \quad (7{\cdot}36)$$

This is precisely the same as on Sommerfeld's theory.

The quantum conditions may be written in the form

$$\int p_r dr=n_1N,\quad \int_0^{2\pi} p_\theta\,.\,d\theta=n_2N \quad \ldots\ldots\ldots \quad (7{\cdot}41)$$

where n_1 and n_2 are integers and N is Planck's constant,

$$\left.\begin{array}{ll} \text{where the radial momentum} & p_r=m_0\dfrac{dr}{d\tau} \\ \text{and the azimuthal momentum} & p_\theta=m_0r^2\dfrac{d\theta}{d\tau} \end{array}\right\} \quad \ldots\ldots \quad (7{\cdot}42)$$

As in Sommerfeld's Paper (3), we obtain the equations

$$\left.\begin{array}{l} n_1N=2\pi p_\theta \\ n_2N=2\pi\kappa p_\theta\left\{\dfrac{1}{\sqrt{1-\varepsilon^2}}-1\right\} \end{array}\right\} \quad \ldots\ldots \quad (7{\cdot}43)$$

whence

$$1-\varepsilon^2=\frac{n_1^2\kappa^2}{(n_1\kappa+n_2)^2} \quad \ldots\ldots\ldots \quad (7{\cdot}44)$$

On calculating the value of W at the perihelion ($\theta=0$, $l\,.\,u=1+\varepsilon$) we find that sinie, r is a minimum,

$$\frac{dr}{dt}=0$$

while

$$r^2\left(\frac{d\theta}{d\tau}\right)^2=r^2\left(\frac{h}{r^2}\right)^2=h^2u^2 \text{ from } (7{\cdot}32)$$

therefore

$$\begin{aligned} W&=m_0V+\frac{1}{2}m_0\left\{\left(\frac{d\theta}{d\tau}\right)^2+r^2\left(\frac{d\theta}{dt}\right)^2\right\} \\ &=\frac{eE}{2e}(1-\varepsilon^2)-\frac{m_0c^2}{2} \\ &=\frac{2\pi^2m_0e^2E^2}{N^2}\,.\,(n_1\kappa+n_2)^{-2}-\frac{m_0c^2}{2} \quad \ldots\ldots\ldots \quad (7{\cdot}5) \end{aligned}$$

from equations (7·36) and (7·44).

Let R denote the Rydberg constant $\dfrac{2\pi^2m_0e^2E^2}{N^3}$, and α the dimensionless number,

$$\left(\frac{2\pi eE}{Nc}\right)^2=\left(\frac{n_1eE}{m_0hc}\right)^2 \quad \ldots\ldots\ldots \quad (7{\cdot}51)$$

and is approximately $5{\cdot}32\times10^{-5}$ for hydrogen.

Then $(n_1\kappa+n_2)^{-2} \doteqdot (n_1+n_2)^{-2}.\left\{1+\frac{\alpha}{n_1(n_1+n_2)}\right\}$ from (7·34) and (7·51), neglecting squares of α. Hence

$$W+\frac{m_0c^2}{2}=\frac{RN}{(n_1+n_2)^2}\left\{1+\frac{\alpha}{(n_1+n_2)^2}.\left(1+\frac{n_2}{n_1}\right)\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7\cdot52)$$

Sommerfeld's formula is

$$W=\frac{RN}{(n_1+n_2)^2}\left\{1+\frac{\alpha}{(n_1+n_2)^2}.\left(\frac{1}{4}+\frac{n_2}{n_1}\right)\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7\cdot53)$$

By comparing these formulæ we observe that the "relativistic correction for circular orbits" namely,

$$\frac{RN\alpha}{(n_1+n_2)^4}.\frac{1}{4}$$

is increased fourfold in equation (7·52). The variations in the "effective Rydberg constant" for different lines in the Balmer series due to this cause have not yet been measured sufficiently accurately to permit any comparisons with theory.

The fine-structure of the series is governed entirely by the term

$$\frac{RN\alpha}{(n_1+n_2)^4}.\frac{n_2}{n_1}$$

and this appears in both formulæ (7·52) and (7·53). Hence the details of the fine-structure will be identical on both theories.

§8. CONCLUSION.

We have shown that the simple assumption made in paragraph 2 connecting the mass of a body with its potential energy, when supported by certain subsidiary hypotheses, is sufficient to account for the following phenomena—the secular advance of the perihelion of the planet Mercury, the deviation of light rays by the sun, the variation of electronic mass with velocity, and the fine-structure of the spectra of hydrogen and of ionised helium. From this it appears that the crucial experiments to test relativistic theories have yet to be devised and performed, as the present theory is sufficient to predict all those phenomena which have hitherto been regarded as the special preserve of the theories of relativity.

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DISCUSSION.

Prof. A. S. **Eddington** (communicated) : I have had the opportunity of reading Dr. Temple's Paper. I have no quarrel with his mathematics, and have often admired the elegance and ingenuity of his methods. But it seems to me that work of this kind can only appeal to someone who has already been led to believe that the relativity theory is fallacious. Otherwise I cannot imagine why anyone should reject the complete physical theory of Einstein, and prefer the method proposed by Dr. Temple (and others) of an arbitrary tinkering of the fundamental equations. It seems to me that Einstein's method, which consists in rejecting certain hypotheses which had crept into current physics without any experimental support, and re-examining our knowledge unbiased by these traditional hypotheses, goes deeper and is more convincing than any proposal to patch up the Newtonian equations by addition of extra terms.

I do not think that the success in explaining " crucial " phenomena is greater than would be expected from the additional constants introduced. It would be interesting if a term added to explain change of mass with velocity were found also to explain the gravitational deflection of light. But it does not. It gives only half the observed deflection ; and the discrepancy has to be cured by arbitrarily doubling the term. The displacement of the Fraunhöfer lines is not explained ; Dr. Temple tackles it by a procedure justified if the relativity theory is accepted, but admittedly unjustified on his own theory, and obtains a result which the experimental evidence (such as it is) does not seem to favour. The fine structure of the hydrogen lines is scarcely an *additional* success ; the discussion of this phenomenon was required in order to remove an apparent difficulty in Dr. Temple's theory which does not arise in Lorentz's or Einstein's theory. I think the one distinct success is in deriving the perihelion of mercury and the deflection of light from the same formulation of the equations ; but even here it seems that we are required to accept a conclusion of the relativity theory (whilst rejecting the arguments that led to it). Surely the discovery that gravitation acts on light-waves compels us to reject the Newtonian picture of gravitation as a force—you cannot deflect *waves* by tugging at them ; Newton's picture of the invisible agent that pulls down the apple does not correspond to an agency which can deviate a train of waves. We have to find (as Einstein has done) a new picture of the gravitational field which shall represent its action on waves as well as on particles ; and it is this new picture which gives us the hint as to the natural formulation of the law of gravitation.

Mr. F. E. **Smith** welcomed the Paper as being a good deal easier to follow than most Papers on relativity. He noted that at one point the author obtained a numerical result different from Einstein's, so that it might subsequently become possible to decide between the two theories experimentally.

Dr. D. **Owen** : The author has set out to provide a theory more congenial to physicists than Einstein's. But the effort can scarcely be deemed successful for the reason that it appears to amount to little more than a re-presentation of the theory of Einstein. Thus, the fundamental equation (2·1) of the Paper is simply a statement of the identity of energy and mass, which is one of the remarkable simplifications flowing out of the Einstein postulates. Again, in equation (2·7), the author has to introduce a " proper time," which surely will not strike the physicist as more congenial than the notion of a time specification related to space. In dealing with the shift of the Fraunhöfer lines the author refers rather contemptuously to the " naïve procedure " of assuming a certain behaviour of " atomic clocks," and then proceeds to help himself to the same artifice ! The fact that he arrives at a shift differing in value from Einstein's appears to have no significance, for this merely depends on the choice he has made, apparently quite arbitrarily, of the numerical values of the coefficient λ. Will the author explain why he has adopted different values for this coefficient in the cases of gravitational and electrical fields ? In view of the avowed aim to provide something more acceptable to physicists than Einstein, it would be of interest to learn whether the Author can bring back the æther ; if so, many will doubtless heave a sigh of relief, though there are not a few who have now learnt to resign themselves to the alternative conception of space-time. Whilst offering these criticisms, I nevertheless think this able Paper of great value as holding out the possibility of the proposition of the identity of energy and mass affording a centre from which many of the conclusions of the relativity theory may be readily derived.

Mr. J. E. **Calthrop** : I do not see how the theory can interpret the experiments of Kaufmann *and* Bucherer. Kaufmann's experiments supported Abraham's formula, but according to Sommerfeld these results were not sufficiently accurate to distinguish between the Abraham and Lorentz expressions. Later work (Schäfer and Neumann, Ann. d. Phys., 45, 529, 1914) has supported the Lorentz and relativity result. A reference will be found in Sommerfeld's "Atomic Structure."

Dr. J. S. G. **Thomas** said that in introducing his Paper the author had referred to D. C. Miller's results, and to Dr. Silberstein's letter in Nature describing these as an invalidation of Einstein's theory. Miller's results should, however, be accepted with great caution until more was known about their experimental details, for, as Prof. Eddington had pointed out, the differential ether drift which they imply is entirely contradicted by ordinary astronomical experience. Dr. Owen seemed to be right in asserting that the author's theory is identical in principle with Einstein's. In ordinary dynamics one can write down either the equations of motion of a particle or its energy equations, and the difference between the two methods appeared to be analogous to the difference between the author's and Einstein's methods, so far as essential principles were concerned. It was true that the author makes only one modification of Newtonian mechanics, but that modification contains by implication the whole of Einstein's theory. There might be other lines of approach, such as that which has recently been put forward by Fournier, but all these lines are equivalent to one another in principle.

Mr. T. **Smith** said that he felt the author's offering to the experimental physicist was not to be despised. He had offered something which could be envisaged, and was of a familiar character—namely, a mode of behaviour of mass and potential energy—whereas Einstein had dealt in terms of action, which is a far more elusive conception.

Mr. F. E. **Smith** said that, after hearing the Paper, he felt he could stand relativity a little better than before. It was gratifying to feel that mathematical physicists could not get much further until experimental physicists supplied them with more ammunition.

Capt. C. W. **Hume** (communicated subsequently): The metaphysical significance of the Paper seems to lie in the fact that it further delivers us from those strange paradoxes concerning the nature of space and time with which Einstein has troubled our peace. To any one who holds an idealist view of the universe a dispute as to the reality of the ether is a dispute about words, for ether and matter are in any case mere intellectual fictions which (like the virtual image in optics) serve to co-ordinate the elements of our experience. But when we are asked by Minkowski's famous dictum to believe that space and time are in some way identical in their nature, that is a very different matter. It has happened before now, as in the case of Carnot's theorem, that the mathematical structure of a theory has had to be separated from the metaphysical content in association with which it was first put forward: it is possible for the former to be true while the latter is false. The author has shown that the mathematical structure of Einstein's theory can be retained in essentials while an entirely different metaphysic, his or another, is associated with it. May I also venture to suggest that Minkowski's dictum about the identity of time and space is based on a fallacy which arises from taking in a literal sense the loose statement that the time-space systems of two relatively moving *observers* will be different from one another. As a matter of fact, the nature of the universe is precisely the same for all observers, even on the principle of relativity; the orientation of the time-space axes is different for different *systems of measurement*, but any observer can choose any system of measurement he pleases. In the loose phraseology commonly employed, each observer is presumed to use that particular system of measurement which in practice it would be most convenient for him to use; but there is no reason in principle why he should not measure time by a flying clock and measure space with high-speed measuring rods, if he chooses. In connexion with the metaphysics of relativity it is worth noting that the time order of events in the experience of any one monad is the same "for all observers"; or, rather, whatever space-time co-ordinates you adopt, the experiences of any monad will occur in the same order, and this order is the only thing in common between clock measurements and our consciousness of time. And, secondly, any statement which contradicts our immediate intuitive judgments must be fallacious, because the statement itself must at best rest ultimately on intuitive judgments which themselves have no greater authority than these. Now if there is any intuitive judgment that is clear and certain it is this, that time and space are two entirely different conceptions, having nothing in common except a remote analogy between order in space and order in time. Hence, if any theory contradicts this fact, either that theory is untrue or it involves an antinomy in the very nature of thought.

Author's reply: The very kind criticism of this Paper which I have received from Prof. Eddington encourages me to point out an aspect of the subject which appears to have escaped notice. I would be the last to deny the cogency of the observations which Prof. Eddington has made, but I have the uneasy feeling that they tell equally against relativistic theories. Thus, although Einstein's work is spoken of as a "complete physical theory," based on the rejection of certain unverified hypotheses, a perusal of such a work as Prof. Whitehead's "Principle of Relativity," with its four sets of gravitational equations, shows that there is an arbitrary element in the selection of the Einsteinian gravitational equation $G_{mr}=0$; while the methods adopted by all relativists in reformulating the equations of the electromagnetic field are frankly selected in order to obtain Maxwell's equations as a first approximation. Such a procedure may not unjustly be described as "tinkering with the fundamental equations." Again, arbitrary constants and even, *horresco referens*, arbitrary functions are not entirely unknown in the General Theory of Relativity. Thus Combridge (Phil. Mag., Vol. 45, pp. 726-732) has shown that the direct integration of the Einsteinian equations for a field possessing spherical symmetry leads to the result (for the line-element of the dynamical manifold)—

$$ds^2=c^2\left(1-\frac{a}{f}\right)dt^2-\left(1-\frac{a}{f}\right)^{-1}.\left(\frac{df}{dr}\right)^2dr^2-f^2(d\theta^2+\sin^2\theta\,.\,d\varphi^2),$$

where a is an arbitrary constant, conveniently identified with $2\gamma M/c^2$ (where γ=the constant of gravitation, c=velocity of light in vacuo, and M=mass of central attracting mass), and f is any arbitrary function of r the radius vector (satisfying two boundary conditions), usually conveniently identified with r itself. There is necessarily an element of contingence in the fundamental equations of any dynamical theory, and all that the present theory can claim is a certain simplicity and intelligibility in its formulation, and a certain agreement with experience in its conclusions. In reply to Dr. Owen, I wish to emphasise the fact that although there is a certain similarity in form between the methods of the present Paper and Einstein's theories, there is a radical diversity of principle. Thus, although equation (2·7) introduces a new variable "τ," named, perhaps somewhat unfortunately, "the proper time," I have deliberately refrained from attaching to this variable any physical significance, such as Minkowski would have attached to the variable bearing the same name in his interpretation of the Special Theory of Relativity ; and I have explicitly stated that the one purpose served by the introduction of this variable is the expression of the equations of motion in Newtonian form—a matter of mathematical convenience rather than of metaphysical necessity. Again, the "identification of mass and energy" in Einstein's theory, alluded to by the same speaker, is a loose, if useful, contraction of a deduction from the Special Theory of Relativity, more accurately expressed in the statement that a flux of kinetic energy w with velocity v is accompanied by a momentum $g=w.v/c^2$ (*see* Chapter XIII., "The Principle of Relativity," E. Cunningham, Camb. Univ. Press, 1914). The deduction assumes that the group of transformations admitted by the equations of particle dynamics is isomorphic with the Lorentz transformation. The assumption of the present Paper is distinct from this result of Einstein's, both in origin and in expression, for it does not presuppose or imply the Lorentz transformation, neither does it contain any reference to kinetic energy. Finally, it appears that both Dr. Owen and Dr. Thomas confuse the special and general theories of relativity. The assumption of that particular consequence of the special theory of relativity, to which allusion has just been made, cannot be used to predict the phenomenon of § 3, neither by itself, nor when supported by the supplementary hypotheses of Poincaré (pp. 173-180, loc. cit.), or of Silberstein (Phil. Mag., Vol. 36., pp. 94-128). The advance of the perihelion of mercury has been explained on relativistic principles, only by the general theories of Einstein and Whitehead, the assumptions of which are destructive of the presuppositions of the present Paper regarding the independence of space and time.

The method of §5 was intended as a *reductio ad absurdum* of the method used in Einstein's theory for predicting the solar spectral shift, and like that method involves an additional *ad hoc* assumption for which no theoretical reason is given, and leads to a result not confirmed by experiment. This criticism is based on the work of Whitehead ("The Principle of Relativity," Camb. Univ. Press, 1922, Chaps. VIII. and XIII.-XV.), who points out that, since electromagnetic forces will be affected by the gravitational field, some knowledge of molecular structure is required for an investigation of the spectral shift.

The theory outlined above implies the negation of an æther only in so far as the existence of an ether is incompatible with the Quantum Theory used in §7. The necessity for using different values in λ in gravitational and electromagnetic problems undoubtedly mars the elegance of the theory. I do not know of any *à priori* justification for these arbitrary values.

In reply to Mr. Calthrop, it appears from pp. 151-3 of Cunningham's textbook quoted above, that although Kaufmann was himself originally of the impression that his experiments supported Abraham's formula, a recalculation of his work, based on a more accurate value of e/m^0, favours the formula of Lorentz.

XXIV.—THE ELECTROSTATIC CAPACITY OF TWO SPHERES WHEN TOUCHING ONE ANOTHER.

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Received May 22, 1925.

ABSTRACT.

Formulæ are given to enable the electrostatic capacity of two unequal spheres in contact to be computed more easily. It is shown that the natural logarithm of any number n can always be expressed as the sum of the capacities of a limited number of pairs of spheres in contact, the number of pairs never being greater than $n/2$. For instance log 7 is the sum of the capacities of the pairs of spheres whose radii are $(1, \frac{1}{6})$, $(\frac{1}{2}, \frac{1}{5})$, and $(\frac{1}{3}, \frac{1}{4})$ respectively. Several similar relations are pointed out. An exact formula is given for the capacity of the pair of spheres, whose radii are a and b respectively. The formulæ given are a help when computing the capacity coefficients of unequal spheres when very close together.

MATHEMATICAL formulæ for the capacities of two spheres when in contact with one another have been known for many years.* If the radii of the spheres be a and b respectively, so that $a+b$ is the distance between their centres, then, if $C_p\,(a, b)$ denote their joint capacity, and if $C_a\,(a, b)$ denote the capacity of the sphere whose radius is a when in contact with the sphere whose radius is b, we have

$$C_p(a, b)=\frac{ab}{a+b}\left\{-\psi\left(\frac{a}{a+b}\right)-\psi\left(\frac{b}{a+b}\right)-2\gamma\right\} \quad . \quad . \quad . \quad . \quad (1)$$

$$C_a(a, b)=\frac{1}{2}C_p(a, b)+\frac{\pi ab}{2(a+b)}\cot\frac{\pi b}{a+b} \quad . \quad . \quad . \quad . \quad (2)$$

and

$$C_b(a, b)=\frac{1}{2}C_p(a, b)+\frac{\pi ab}{2(a+b)}\cot\frac{\pi a}{a+b} \quad . \quad . \quad . \quad . \quad (3)$$

where $\psi(x)$ is the logarithmic derivate of the gamma function and γ is Euler's constant (0·577 215 7 . . .). Series formulæ for $\psi(x)$ are given in Todhunter's "Integral Calculus," and a short table of their values is given in Gauss's "Werke," Band 3, pp. 161-162.

As it is now possible to measure the capacity of even a small conductor with high accuracy, the following formula for $C_p\,(a, b)$ will be found useful.

When a is not less than b, we have

$$C_p(a, b)=a\left\{1+\frac{2b^3}{a(a+b)\,(a+2b)}+0{\cdot}40412\left(\frac{b}{a+b}\right)^3 \right.$$
$$\left. +0{\cdot}07386\left(\frac{b}{a+b}\right)^5+0{\cdot}0167\left(\frac{b}{a+b}\right)^7+0{\cdot}004\left(\frac{b}{a+b}\right)^9\right\} \quad . \quad . \quad . \quad . \quad (4)$$

approximately. This formula can be proved at once from the ordinary series formula for $\psi(x)$. Its maximum inaccuracy occurs when a equals b. In this case, putting $a=b=1$, we get

$$C_p\,(1, 1)=1{\cdot}386\ 294\ 7\ .\ .\ .$$

The true value $=2 \log 2=1{\cdot}386\ 294\ 4\ .\ .\ .$

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* A. Russell, Alternating Currents, Vol. 1, p. 243.

As another example, put $a=1$, $b=\frac{1}{3}$, then by (4), (2) and (3) we have

$$C_p\left(1, \frac{1}{3}\right)=1{\cdot}039\ 738\ 8\ \ldots$$

$$C_1\left(1, \frac{1}{3}\right)=\frac{1}{2}C_p\left(1, \frac{1}{3}\right)+\frac{\pi}{8}=0{\cdot}912\ 559\ 5\ \ldots$$

$$C_{\frac{1}{3}}\left(1, \frac{1}{3}\right)=\frac{1}{2}C_p\left(1, \frac{1}{3}\right)-\frac{\pi}{8}=0{\cdot}127\ 179\ 3\ \ldots$$

If we divide these numbers by 900 000 we get the capacities in microfarads. Formulæ (2), (3) and (4) give a practical solution of the problem.

In many cases, however, a simple exact formula can be given for $C_p\ (a, b)$ in terms of logarithmic and trigonometric functions. In proving this formula curious and interesting relations have been found connecting the sums and differences of the electrostatic capacities of pairs of spheres.

If we differentiate Gauss's Multiplication Theorem for gamma functions, we find that

$$\psi(x)+\psi\left(x+\frac{1}{n}\right)+\psi\left(x+\frac{2}{n}\right)+\ \ldots\ +\psi\left(x+\frac{n-1}{n}\right)=n\,\psi(nx)-n\log n \quad . \quad . \quad (5)$$

Putting $x=\frac{1}{n}$ and using (1), we get the following theorems in electrostatics :—

When n is an even integer,

$$C_p\left(1, \frac{1}{n-1}\right)+C_p\left(\frac{1}{2}, \frac{1}{n-2}\right)+\ \ldots\ +C_p\left(\frac{2}{n-2}, \frac{2}{n+2}\right)$$
$$+\frac{1}{2}C_p\left(\frac{2}{n}, \frac{2}{n}\right)=\log_e n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

And when n is odd,

$$C_p\left(1, \frac{1}{n-1}\right)+C_p\left(\frac{1}{2}, \frac{1}{n-2}\right)+\ \ldots\ +C_p\left(\frac{2}{n-1}, \frac{2}{n+1}\right)=\log_e n \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Putting $n=2, 4, 6\ \ldots$ in (6) we get

$$C_p\left(\frac{1}{2}, \frac{1}{2}\right)=\log 2\ ;\ C_p\left(1, \frac{1}{3}\right)+C_p\left(\frac{1}{4}, \frac{1}{4}\right)=\log 4\ ;$$

$$C_p\left(1, \frac{1}{5}\right)+C_p\left(\frac{1}{2}, \frac{1}{4}\right)+C_p\left(\frac{1}{6}, \frac{1}{6}\right)=\log 6\ ;\ \text{etc.}$$

Similarly, putting $n=3, 5, 7, \ldots$ in (7), we have

$$C_p\left(1, \frac{1}{2}\right)=\log 3\ ;\ C_p\left(1, \frac{1}{4}\right)+C_p\left(\frac{1}{2}, \frac{1}{3}\right)=\log 5\ ;$$

$$C_p\left(1, \frac{1}{6}\right)+C_p\left(\frac{1}{2}, \frac{1}{5}\right)+C_p\left(\frac{1}{3}, \frac{1}{4}\right)=\log 7\ ;\ \text{etc.}$$

It is curious that the natural logarithm of any number n can always be expressed as the sum of

$$C_p\left(1, \frac{1}{n-1}\right), C_p\left(\frac{1}{2}, \frac{1}{n-2}\right), \ldots$$

the number of pairs of spheres never being greater than $n/2$. Similarly, by writing $1/(2n)$ for x in (5), we get, when n is even,

$$C_p\left(1, \frac{1}{2n-1}\right)+C_p\left(\frac{1}{3}, \frac{1}{2n-3}\right)+ \ldots +C_p\left(\frac{1}{n-1}, \frac{1}{n+1}\right)=\frac{1}{2}\log 4n \quad . \quad . \quad . \quad . \quad (8)$$

And when n is odd,

$$C_p\left(1, \frac{1}{2n-1}\right)+C_p\left(\frac{1}{3}, \frac{1}{2n-3}\right)+ \ldots +\frac{1}{2}C_p\left(\frac{1}{n}, \frac{1}{n}\right)=\frac{1}{2}\log 4n \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Many similar theorems can be deduced from the theorems in connexion with $\psi(x)$ given in Bertrand's "Integral Calculus." It suffices to give the following formula:—

$$C_p\left(\frac{1}{n}, \frac{1}{m-n}\right)=\frac{2}{m}\log 2m-\frac{4}{m}\cos\frac{2\pi n}{m}\log\left(\sin\frac{\pi}{m}\right)$$
$$-\frac{4}{m}\cos 2\frac{2\pi n}{m}\log\left(\sin\frac{2\pi}{m}\right)- \ldots -\frac{4}{m}\cos q\frac{2\pi n}{m}\log\left(\sin\frac{q\pi}{m}\right) \quad . \quad (10)$$

where q is $\frac{m}{2}-1$ when m is even and $(m-1)/2$ when m is odd. For example, when $m=6$ and $n=1$, (10) simplifies to

$$C_p\left(1, \frac{1}{5}\right)=\frac{1}{2}\log 6+\frac{1}{6}\log 2=1{\cdot}011\ 404\ 3 \ldots$$

To this accuracy we get the same value by (4).

Notice that

$$C_p\left(m, \frac{m}{5}\right)=\frac{1}{2}\log 6^m+\frac{1}{6}\log 2^m.$$

The theorems given above show that there are numerous simple relations connecting the capacities of pairs of spheres which touch one another. For instance, we can easily prove that

$$\begin{aligned}\log 4&=C_p(1, 1)=C_p\left(\frac{4}{3}, \frac{4}{9}\right)=C_p\left(1, \frac{1}{7}\right)+C_p\left(\frac{1}{3}, \frac{1}{5}\right)\\&=C_p\left(1, \frac{1}{3}\right)+C_p\left(\frac{1}{4}, \frac{1}{4}\right)=C_p\left(2, \frac{2}{11}\right)+C_p\left(\frac{2}{5}, \frac{2}{7}\right)-C_p\left(1, \frac{1}{2}\right)\\&=C_p\left(3, \frac{3}{5}\right)-C_p\left(\frac{3}{2}, \frac{3}{4}\right)\\&=C_p\left(\frac{2}{1}, \frac{2}{2n-1}\right)-C_p\left(\frac{2}{2}, \frac{2}{2n-2}\right)+C_p\left(\frac{2}{3}, \frac{2}{2n-3}\right)\\&\quad- \ldots + (-)^{n+1}C_p\left(\frac{1}{n}, \frac{1}{n}\right),\end{aligned}$$

where n is any integer greater than unity.

The following particular cases follow from (10) :—

$$C_p(1\,,\,1)=\log 4 \qquad C_p\left(1\,,\frac{1}{5}\right)=\frac{1}{2}\log 6+\frac{1}{6}\log 2$$

$$C_p\left(1\,,\frac{1}{2}\right)=\log 3 \qquad C_p\left(1\,,\frac{1}{7}\right)=\log 2+\frac{\sqrt{2}}{4}\log(1+\sqrt{2})$$

$$C_p\left(1\,,\frac{1}{3}\right)=\frac{1}{2}\log 8$$

$$C_p\left(1\,,\frac{1}{4}\right)=\frac{1}{2}\log 5+\frac{2\sqrt{5}}{10}\log\frac{1+\sqrt{5}}{2} \qquad C_p\left(1\,,\frac{1}{9}\right)=\frac{1}{5}\log 4+\frac{1}{4}\log 5+\frac{3\sqrt{5}}{10}\log\frac{1+\sqrt{5}}{2}$$

$$C_p\left(1\,,\frac{1}{11}\right)=\frac{1}{4}\log 12+\frac{\sqrt{3}}{6}\log(2+\sqrt{3}).$$

We also have

$$C_p\left(1\,,\frac{1}{11}\right)+C_p\left(\frac{1}{5}\,,\frac{1}{7}\right)=\frac{1}{2}\log 12$$

$$C_p\left(1\,,\,\frac{1}{7}\right)+C_p\left(\frac{1}{3}\,,\frac{1}{5}\right)=2\log 2,$$

and

$$C_p\left(1\,,\,\frac{1}{4}\right)+C_p\left(\frac{1}{2}\,,\frac{1}{3}\right)=\log 5.$$

We thus find $C_p\left(\frac{1}{5}\,,\frac{1}{7}\right)$, $C_p\left(\frac{1}{3}\,,\frac{1}{5}\right)$ and $C_p\left(\frac{1}{2}\,,\frac{1}{3}\right)$.

A knowledge of the values of $C_p(a, b)$ and $C_a(a, b)$ is a help in computing the capacity coefficients k_{11}, k_{22} and k_{12} of the two spheres when very close together. In this case we have

$$k_{11}+k_{22}+2\,k_{12}=C_p(a, b)$$
$$k_{11}+k_{12}=C_a(a, b)$$

very approximately, and k_{12} can be found from the known formula for two equal spheres, by means of the formula*

$$k_{12}\,(a, b, c)=k_{12}(a_1, a_1, c_1) \qquad \text{. . . . .} \quad (11)$$

where

$$a_1^2=ab\left\{1-\frac{(a-b)^2}{c^2}\right\} \text{ and } c_1=c\left\{1-\frac{(a-b)^2}{c^2}\right\}.$$

DISCUSSION.

Dr. RUSSELL, in introducing the Paper: I think some explanation is necessary for bringing a mathematical theorem to the notice of the Society. I have found that the computation of the maximum stress in the dielectric between unequal spherical electrodes when electrified, and also of the attraction between them, is very laborious. A knowledge of the values of these quantities is of great importance to the physicist. For instance, with equal spherical electrodes in air the maximum electric stress at the moment of disruptive discharge is a constant at a given temperature and pressure. I would instance the sparking voltages between large spheres which are published annually by the American Institute of Electrical Engineers. In the experiments where the potentials are equal and opposite at the moment of discharge I have found that at 25°C. and 76 cm. pressure the disruptive electric stress is given by $27{\cdot}4+14{\cdot}1/\sqrt{r}$ kv. per cm., where r is the radius of either electrode in cm. I have also found that this expression enables the experimental results found by Heydweiller in 1893 for the sparking voltages between very small spherical electrodes to be accurately computed. In general, for a fixed spark-gap there are certain sized spherical electrodes which offer the maximum resistance to sparking. For example, when the air-gap is 1 cm. the sparking voltage is a maximum for spherical electrodes whose diameters are very nearly equal to two inches. Increasing or diminishing the size of the electrodes from this value makes the sparks take place more readily. It is of interest both practically and theoretically to analyse the sparking voltages between unequal electrodes. A necessary preliminary is to find their capacity coefficients, and as an aid to calculating their values this theorem is published.

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* Proc. Roy. Soc., Vol. 97, p. 166.

Dr. E. H. **Rayner** : I had an opportunity recently of visiting the transformer works of the General Electric Company of America at Pittsfield, Mass., where much of the work has been done by Mr. Peek and others which has resulted in the adoption of the American figures. As far as I am aware, the figures are derived from an empirical formula made to fit a few points of an experimental character. The accurate measurement of voltages of the order of a few hundred kilovolts, both peak voltages and effective voltages, has, I think, hardly reached a stage at which definite information has been obtained as to the accuracy with which a formula, empirical or otherwise, actually corresponds with the physical facts. The statement of Dr. Russell that a discharge might take the more difficult of two parallel spark gaps is an illustration of the effect of the physical conditions on the relations involved. This actual experiment was shown at Pittsfield, in which two spark gaps two or three inches long were arranged in parallel. The electrodes of one were spheres about four inches in diameter, and the other was a horn type lightning arrester made of rods about half an inch in diameter. At ordinary power frequencies the gaps can be adjusted so that the discharge takes place at the horn type of gap, while at high frequency or impulsive discharges it will change over to the other gap. The reason for this seems to be that at power frequencies there is time for corona to form at the horn gap and ionise the air, and so assist the discharge, while at high frequencies there is not sufficient time for this to be fully effective, and the discharge takes the other path.

Author's reply ; The experiment with parallel spark gaps was shown me by Mr. Duddell many years ago. The explanation was fairly obvious. I am not prepared to say that the formula for the disruptive stress is an empirical formula : it is necessary first of all to compute the maximum electric stress, and this involves a large amount of theory. Experiments carried out in many different laboratories during the last forty years have proved that with certain limitations the disruptive stress is a constant for a given pair of equal spherical electrodes. This physical fact was anticipated by the early physicists : when, however, it was found that the disruptive stress varied with the size of the spheres it became desirable to study the problem more closely. From considerations on the convective discharge of electricity I was led to the conclusion that an expression of the form $A+B/\sqrt{r}$, where A and B are constants, would give the disruptive stress, and Whitehead's experiments on the disruptive voltage between parallel cylinders were the first to give the theory strong experimental support. So strong, in fact, is the experimental evidence that I have not the slightest hesitation in applying it in practice, provided that there are no brush discharges in the neighbourhood of the spark gap.

XXV.—CONDENSATION NUCLEI PRODUCED BY THE ILLUMINATION OF AIR-HALOGEN MIXTURES.

By I. C. JONES, *B.Sc.*, Garrod Thomas Research Student, University College, Aberystwyth.

Received March 18, 1925.

ABSTRACT.

It is shown that the production of condensation nuclei, previously known to take place on illumination of air saturated with iodine vapour, also takes place under similar conditions with bromine and chlorine; the usual tests indicate that the nuclei in all three cases are similar.

The effect occurs in two distinct stages:

(1) A surface action dependent on the presence on the surface of traces of some oxidisable substance.

(2) Formation of nuclei in the "activated" vapour by illumination.

A parallel is shown to exist between the conditions under which nuclei are found, and those under which an induction period is observed in the hydrogen-chlorine reaction.

INTRODUCTORY.

THE present investigation is a continuation of work on the same subject by G. Owen and H. Pealing,* and by H. Pealing.† Much of their work has been repeated and in general confirmed, but a more detailed and definite explanation of the results has been obtained.

The problem arose in the following way: It had been found by G. Owen and A. Ll. Hughes‡ that CO_2 snow, previously condensed in a dust-free state, gave rise on vaporising to immense numbers of particles capable of acting as centres of condensation for water vapour. Similar tests with other subliming substances gave in general negative results, but in the case of iodine a large effect was observed. Later, however, it was shown that in this case the nuclei were not formed if the apparatus was kept in darkness, so that the process producing them was not mechanical, but photochemical.

The apparatus used in the present experiments was the well-known expansion apparatus due to C. T. R. Wilson,§ with which different forms of cloud-chamber could be used as convenience and necessity indicated.

The results produced in saturated dust-free air by expansions of various amounts are so well known that they need not be described in detail; in the present Paper they will be referred to as the "Wilson effects." If the initial and final volumes be respectively v_1 and v_2, B the atmospheric pressure, π the pressure of water vapour at the prevailing temperature, and p_1 and p_2 the initial and final readings of a manometer attached to the expansion chamber, then

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* G. Owen and H. Pealing, Phil. Mag., p. 465, April (1911).

† H. Pealing, Phil. Mag., p. 455 (1915).

‡ G. Owen and A. Ll. Hughes, Phil. Mag., October (1907); June (1908).

§ C. T. R. Wilson, Proc. Camb. Phil. Soc., 9, p. 333 (1897).

$$\frac{v_2}{v_1}=\frac{B-p_1-\pi}{B-p_2-\pi}$$

It is usually convenient to denote the measure of the expansion by the pressure-fall p_2-p_1 produced by it; the calculation of the expansion ratio v_2/v_1 is only occasionally necessary.

THE PHOTOCHEMICAL EFFECT.

For the observing of the effect, the type of cloud-chamber shown in Fig. 1 was found convenient. A few crystals of iodine were placed in the cup A, and, after freeing the air from dust, expansions were made, with the results shown below. The expansions " In Light " were made with the apparatus illuminated by a 100-watt lamp about five feet away; to view the clouds, this light was switched off and a second light put on, so arranged as to send a narrow beam of light through the cloud-chamber, the effects being observed at a small angle to this beam, against a black background. The results of expansions made with the apparatus in darkness are added for comparison.

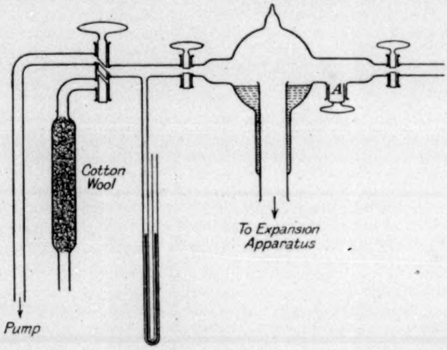

FIG. 1.

Pressure-fall (from atmospheric).	*Result of expansion.* *In light.*	*In darkness (Wilson effects).*
15 cm.	Fair shower.	Few drops.
17 cm.	Coarse-grain cloud.	Medium shower.
18·5 cm.	Dense coloured cloud.	Good shower.
20 cm.	Fog.	Fog.

A greatly increased density of cloud is thus observed at 18·5 cm., and also somewhat more nuclei at smaller pressure-falls. This confirms the result obtained by Owen and Pealing.

The Effect in Bromine and Chlorine.

To test whether the nuclei are peculiar to iodine or common to all the halogens, experiments were made with bromine and chlorine. Bromine was introduced by placing a few drops in the cup *A* ; chlorine was passed in from a cylinder. In order to make a fair comparison, the chlorine was greatly diluted with air ; this approximates to the conditions under which bromine and iodine were examined, and also does not greatly alter the value of Cp/Cv. (Cp/Cv being different for chlorine and air, the same pressure-fall will not produce the same cooling, and therefore the same Wilson effect, in both cases.)

The results are given below ; in both cases the nucleation in darkness was the usual Wilson effect.

	Result of expansion (in light).	
Pressure-fall.	*In Bromine.*	*In Chlorine.*
15·0 cm.	Small shower.	—
16·5 cm.	Coarse cloud.	Coarse cloud.
18·0 cm.	Fine cloud.	—
18·5 cm.	Dense coloured cloud.	Dense coloured cloud.
19·0 cm.	Dense coloured cloud.	Dense coloured cloud.

The same effect is seen to be present in both cases as in iodine. In presence of an excess of chlorine, the difference between the " light " and " darkness " effects was not so great as when the chlorine was greatly diluted with air.

Growth and Decay of Nuclei.

In the case of iodine, it was observed by Owen and Pealing that the nuclei take about one second to " grow " in light—i.e., the full density of cloud at 18·5 cm. is not obtained till the vapour has been illuminated for that time. Similarly, the nuclei take about thirty seconds to disappear after the light has been cut off.

Measurements of the rate of growth and decay were made in bromine and chlorine, and found to be the same as those for iodine, as the following tables show :

Rate of Growth.—Pressure-fall throughout, 18·5 cm.

	Result of expansion.	
Time of illumination.	*In Bromine.*	*In Chlorine.*
About ¼ sec.	Heavy shower.	Good shower.
¾ sec.	Fine cloud.	Coarse cloud.
1 sec.	Dense coloured cloud.	Fine cloud.
2 sec.	Dense coloured cloud.	Dense coloured cloud.

Rate of Decay.—Pressure-fall throughout, 18·5 cm.

	Result of expansion.	
Period of darkness before expansion.	*In Bromine.*	*In Chlorine.*
2 mins.	Shower (Wilson effect).	Shower (Wilson effect).
1 min.	Heavy shower.	Heavy shower.
30 sec.	Very heavy shower.	Very heavy shower.
5 sec.	Dense coloured cloud.	Dense coloured cloud.

It is seen that all the nuclei persist for five seconds, and that some are still present after thirty seconds.

It has now been shown that illumination of a mixture of any one of the common halogens, with air for a period of 1-2 seconds, produces an immense number of nuclei, caught by an expansion of ratio 1·32-1·33 (pressure-fall 18·5-19 cm.), and disappearing entirely if the light is cut off for thirty seconds or longer. The density of the clouds is, as far as can be judged, the same in all three cases. The assumption is therefore justified that the nuclei are similar in all three cases, that they are produced by the same process, and that in all probability the halogen does not enter into their composition.

The diminution of the effect with time.

One point of difference was observed between the halogens, namely, the rate of diminution of the effect with time. Owen and Pealing state that within four or five days of setting up the apparatus the result of an expansion of 18·5 cm. is merely the usual Wilson effect. Their apparatus was kept in bright daylight; if kept in darkness the effect decayed much more slowly. No such complete decay was observed in the present work, but this was probably due to its not being practicable to obtain more than weak diffuse daylight. A marked diminution in the density of the clouds was produced by illuminating the apparatus for some hours with an electric arc.

The rate of decay observed in iodine was very small, a slight diminution being the most observed, even after several weeks.

In bromine the rate of decay was also small, but greater than in iodine, a diminution being evident in six or seven days.

In the case of chlorine the effect had completely disappeared in three days.

Since the effect appears to be practically identical in all three halogens, further work was done with iodine only, this being more convenient to manipulate than bromine or chlorine, which require for prolonged experiments an apparatus constructed entirely of glass.

THE MECHANISM OF THE EFFECT.

Owen and Pealing produced strong evidence to show that the nuclei were produced by some action taking place at the glass surface; after the effect had diminished with time, it could be renewed by taking the apparatus down and washing the walls, whilst introducing fresh air only, without washing the walls, sometimes renewed the effect, but did not generally do so.

It was considered desirable in the present experiments to renew the surface in contact with the vapour without changing the air. For this purpose the apparatus shown in Fig. 2 was used so constructed that the cloud-chamber could be filled with water, this being then replaced by iodine-laden air in which the effect had disappeared. The vessel A containing this air, with iodine in a small glass cage as shown, was exposed to bright daylight for about two weeks, and the air then transferred to the cloud-chamber as indicated, the water being run out at the base of the expansion apparatus.

On testing this " fatigued " air for nuclei, the full effect was found to be present.

As, according to Owen and Pealing's observations, the nucleation in the exposed vessel should have disappeared entirely, owing to the prolonged exposure to bright daylight, the above result goes to prove that bringing the air in which the effect has decayed into contact with a freshly-washed glass surface renews the effect completely.

The production of the nuclei is, then, undoubtedly a surface action. It is noticed, however, that the clouds characterising the effect are produced throughout the whole cloud-chamber. If the nuclei are produced in any way *by* the surface, they should be produced *at* the surface ; so that there should be a greater density of cloud near the surface than at a distance.

It was readily shown that the nuclei occur only in the illuminated parts of the apparatus, by using a cloud-chamber with two bulbs, one of which could be illuminated whilst the other was kept in darkness. The difference in the effects in the two bulbs was very striking, the "dark" bulb being almost optically empty in comparison with the " light " bulb, which was filled with a dense white cloud.

To detect a difference, if any, in the cloud density near the surface and far away, a T-shaped cloud-chamber was constructed, the ends of the horizontal part being ground flat and closed with glass plates. A small beam of light was sent along the axis of this tube, the beam only touching the glass at its entry and exit. On setting, the density of cloud was seen to be the same along the whole path of the beam.

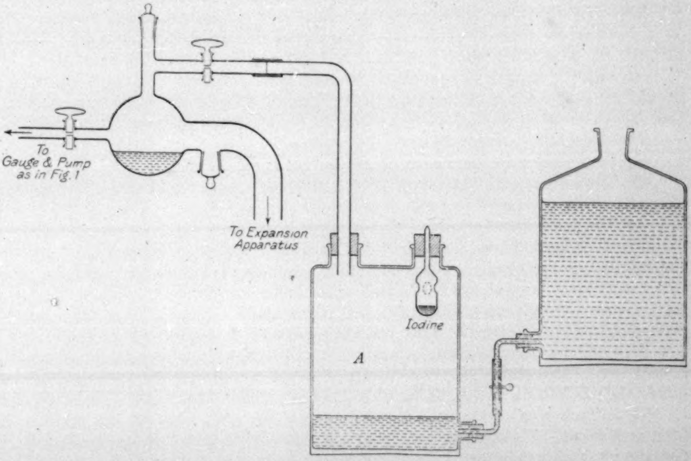

FIG. 2.

Owing to the peculiar shape of this apparatus, a considerable amount of turbulence occurred on expansion. An apparatus was accordingly designed in which the turbulence was eliminated, but the same result as before was obtained.

It seems therefore that the nuclei are produced, not at the surface, but throughout the whole volume of air illuminated.

Owen and Pealing found that if the iodine vapour was passed into the cloud-chamber along a long plug of glass wool, the effect was much more intense than normally : a more detailed examination of this effect of glass wool has explained the above apparent anomaly. The experiments were carried out at a time when the "normal" effect in the cloud-chamber was less than usual, so that the differences in the effects could be detected with certainty.

It may be mentioned in passing that under certain conditions it is found that the glass wool gives rise to large nuclei, caught by expansions of the order of 10-12 cm.; but the property of forming these nuclei very soon disappears, and cannot be renewed by cleaning the glass wool—the nuclei are only formed by a freshly-made plug.

With regard to the small nuclei, the following suggestive result was soon observed. While the apparatus was in the non-sensitive state referred to above, iodine vapour and air were drawn through the glass wool into the cloud-chamber, and the light cut off for a few minutes. Making an expansion of 18·5 cm. in the dark, the result was merely the Wilson effect; putting on the light and expanding again, without drawing more air through the glass wool, gave a dense white fog.

This shows that the nuclei are not produced at the glass surface, or even directly by it, but that the mixture of air and iodine vapour undergoes some change in passing through the glass wool which enables it to form nuclei on being illuminated.

An exactly similar circumstance is observed in the case of the large nuclei caught at 10-12 cm.; it follows that the two kinds of nuclei are of the same nature, and differ merely in size.

The effect, then, separates into two parts:—

(1) Some action in the air-iodine mixture at the glass surface;

(2) The formation of nuclei in this "activated" vapour by light. The surface effect has been examined in some detail:—

(*a*) The dense cloud observed with glass wool becomes less dense on repetition—without drawing more air through the glass wool—until after about 20 expansions the original coarse cloud is obtained. This small effect having been restored, the whole apparatus was allowed to stand in darkness for about 30 minutes, and air then drawn through the glass wool, still in darkness. Expanding at 18·5 cm.—good shower; putting on the light and again expanding—dense white cloud.

It is evident that the glass surface which "activates" the vapour need not be illuminated. This shows why the density of cloud throughout the apparatus is the same; the whole body of the gas has already been activated before the light is put on.

(*b*) A fresh plug of glass wool was made up, the glass wool having previously been well dried. The finished plug was also allowed to stand for some days in a desiccator; freshly-sublimed iodine was placed on it, and dry air drawn through it into the cloud-chamber. Expanding at 18·5 cm. in light—dense white cloud.

This result shows that moisture (other than surface films on the glass, which are not removed without heating in a vacuum) is not necessary to the surface effect.

(*c*) Hydrogen was passed through the glass wool and out to the pump for some time, so as to sweep all air from the glass wool and connecting tubes. Then the tap to the cloud-chamber was opened, and the current of hydrogen allowed to pass in for a short time.

Expanding at 18·5 cm. in light—dense white cloud. The presence of oxygen is therefore not necessary to the surface effect.

(*d*) The effect of the glass wool diminished rapidly with time, disappearing entirely in about three days. Attempts were made to renew it by moistening the plug, cleaning it with NaOH and chromic acid, &c., but none of the varied treatments applied had any appreciable effect.

(*e*) It was observed that if the apparatus was allowed to stand in chromic acid for some time before setting up, the effect in it was less than usual; to see whether the glass wool would be similarly affected a sample of it was allowed to stand for two days in chromic-nitric acid mixture, being afterwards thoroughly washed with water, dried, and made up into a plug. On testing, no appreciable difference could be observed between the clouds produced with and without glass wool.

It appears then, that a thoroughly clean glass surface cannot "activate" the air-iodine mixture.

It was found possible to destroy the "normal" effect in the apparatus itself. Following a suggestion whose origin will appear later, the water to be used in the apparatus was boiled for some time with iodine; the apparatus was allowed to stand for three days in chromic acid, then thoroughly washed, first with tap-water and finally with the "iodised" water.

On testing for nuclei, the effect was found to be exceedingly small—if it was not being deliberately looked for, it would have passed without notice.

As regards the formation of the nuclei, it was found that a trace only of water was required for their formation; once the nuclei are formed, any vapour can condense upon them; for example, in presence of traces of water, the effect is very marked in xylene at pressure-falls of about 16-17 cm.

CONCLUSION.

It cannot be stated with any degree of certainty what the nuclei are. The evidence goes to show that their production is associated with the presence on the surfaces in contact with the vapour, of some foreign substance which is oxidisable, and which after oxidation ceases to exert any action. The decay of the effect is readily explained by the complete oxidation of this substance; the oxidation will be more rapid in the case of chlorine than in iodine—causing a more rapid rate of decay. Also the oxidation will proceed more readily in light than in darkness; with a corresponding effect on the rate of decay, such as is observed. Treatment of the surfaces with chromic acid will oxidise the impurity; any such substance in the water is removed by boiling with the halogen.

The strange manner in which the effect was observed to vary in intensity from time to time is explained by the presence of a varying amount of impurity in the air of the room; the presence of foreign matter on the specimens of glass wool used was readily made evident by heating a fresh plug of wool moderately strongly, when the wool became darkened as though carbonised, and gave rise to a faint odour as of burning organic matter. After treatment with chromic acid the glass wool did not behave in this way.

The action of the glass wool is not regarded as being in any way different from that of the glass walls of the apparatus; the greater intensity of the effect is due to the greater surface and consequently greater amount of foreign adsorbed matter in the case of the glass wool.

There are three possible kinds of surface action which may account for the formation of the nuclei:—

(1) Some action between the iodine and some impurity on the glass.

(2) Some surface action between the glass and iodine or some compound of iodine.

(3) Some catalytic action at the glass surface between iodine, water, and oxygen.

Owen and Pealing considered that the action involved in this case was either (2) or (3), but were unable to reach any definite conclusion. Pealing, in a later Paper, also failed to make any definite statement, though leaning to (3). In this Paper it has been shown fairly conclusively that the surface effect does not take place if the glass is thoroughly clean, so that (1) must represent the correct view.

The view of the effect put forward here coincides to some extent with results obtained by Ramsauer* on the formation of nuclei in air by ultra-violet light. He found that the production of the nuclei was associated with the presence in the air or on the surfaces in contact with it, of small traces of substances like CO_2, NH_3, SO_2, organic vapours, &c.; when these were carefully removed, by a variety of methods, the nuclei were not formed. He considered that the direct cause of the nucleus formation was an interaction between the ozone generated by the light and these substances. Owen and Pealing suggested that the reactions

$$H_2O+I_2=HIO+HI \quad : \quad HIO+O_2=HI+O_3$$

might be taking place in the present case, but this is doubtful.

It is possible that the product of the surface action may absorb visible radiation and re-emit it as a kind of resonance radiation of such a wavelength as to produce ozone from the air, and accordingly nuclei as in the experiments of Ramsauer.

To summarise, the nuclei appear to be the result of a complex process going on in two distinct stages.

(1) Some action, the nature of which is not known, between the halogen and some oxidisable substance existing under ordinary conditions on all glass surfaces; this action takes place in darkness and in the absence of water or oxygen.

(2) The vapour, when moistened and mixed with oxygen, after being in contact with the surface, is in a condition to form an immense number of nuclei of a fairly definite and uniform size on being illuminated for a short time. If the amount of surface exposed to the vapour be very large, as in the case of glass wool, the nuclei may grow to an abnormally large size.

Mention may be made of a parallel which seems to exist between the results of this work and those obtained by a number of investigators in connexion with the "activation" of chlorine by light. It is well known that a mixture of hydrogen and chlorine must be exposed to light for some time, varying according to the conditions from seconds to hours, before combination to form HCl takes place. The phenomenon has been very widely examined, but is as yet far from being fully understood. It has been traced by some to an action of the light on the *chlorine* only; and it is generally found that illuminated chlorine is more chemically active than chlorine kept in darkness.

Weigert† has suggested that when gaseous systems containing chlorine are illuminated, "molecular complexes" are produced, which act catalytically as "reaction nuclei"—in the same way as any heterogeneous catalyst. In support of this he points out that many gaseous reactions which are not themselves sensitive to light can be sensitised by the addition of chlorine, i.e., by providing these nuclear catalysts. The theory is very attractive but is negatived by a considerable body of evidence.

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* Ramsauer, Phil. Mag., 23, p. 852 (1912).

† Weigert, Ann. der Phys., 4, pp. 243-266 (1907).

The formation of nuclei in illuminated chlorine has been observed by Aitken,[*] Bevan,[†] Burgess and Chapman,[‡] Radel[§] (by an ultra-microscopic method), and others. In all cases the nuclei are observed only if the chlorine is moist. Bevan attributed them to the presence of complex molecules of an addition compound $Cl_2.H_2O$, or in the presence of hydrogen, $H_2.Cl_2.H_2O$. Later work by Burgess and Chapman showed this explanation to be untenable; they attribute the cloud-formation not to the presence of any unusual nuclei, but merely to the lowering of vapour-pressure of the water by the HCl formed in the reaction

$$2Cl_2+2H_2O=4HCl+O_2$$

The parallel between the facts with regard to chlorine and the present observations on iodine is based on the supposition that the nuclei found in iodine and chlorine are the same; it appears that the conditions under which nuclei are observed in iodine vapour (or chlorine) coincide very closely with those where an induction period is found in the hydrogen-chlorine reaction.

Some points of similarity are tabulated below:—

NUCLEATION IN IODINE.	INDUCTION PERIOD IN CHLORINE.
1. Water is necessary for the formation of the nuclei.	1. An induction period is observed only when the chlorine is moist.
2. Oxygen is necessary to the nucleus formation.	2. Oxygen is found to act as an "inhibitor," i.e., produces or lengthens the induction period.[‖]
3. Nuclei are not formed when the glass surfaces involved have been thoroughly cleaned.	3. No induction period is observed in thoroughly clean quartz apparatus.[¶]
4. Nuclei are not produced if the water used has been previously boiled with iodine, and the apparatus, cleaned as in (3) worked with this water.	4. No induction period is observed if the water is previously heated at 100 with chlorine to destroy volatile nitrogen compounds.[**]
5. The production of nuclei is prevented by prolonged illumination of the apparatus.	5. Prolonged illumination of the reaction vessel shortens and finally removes the induction period.[††]
6. If there is originally no nucleation in the apparatus, it increases slightly with time.	6. "Inhibitors" are gradually produced in the reaction vessel on allowing to stand.[‡‡]
7. Nuclei are formed in presence of salt solutions.	7. Salt solutions cause an induction period.[§§]

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[*] Aitken, Proc. Roy. Soc. Edin. (1897).

[†] Bevan, Phil. Trans., A., p. 347 (1903).

[‡] Burgess and Chapman, Journ. Chem. Soc., p. 1423 (1906).

[§] Radel, Zeit. Phys. Chem., 95, pp. 378-383 (1920).

[‖] Chapman and Macmahon, Journ. Chem. Soc., p. 135 (1909).

[¶] Burgess and Chapman, Journ. Chem. Soc., p. 1423 (1906).

[**] Burgess and Chapman, loc. cit. Baly and Barker, Journ. Chem. Soc., p. 653 (1921).

[††] Burgess and Chapman, loc. cit. Wendt, Landauer, and Ewing, J.Am.C.S., p. 2377 (1922).

[‡‡] Chapman and Whiston, Journ. Chem. Soc., p. 1264 (1919).

[§§] Burgess and Chapman, loc. cit.

It seems that it is the actual nuclei, and not the surface conditions producing them, which are related to the phenomenon of photochemical induction; the suggestion is inevitable that the induction period is caused by the available light energy being used up in the formation of nuclei until a certain state is reached where the light energy is able to energise the chemical reaction.

From this point of view, the suggestion made by Weigert is untenable, since he assumes the nuclei to be essential to the course of the reaction.

The force of the explanation of the cloud-formation given by Burgess and Chapman is in great part removed by the fact that it does not occur in the absence of oxygen; also the part played by the surfaces involved is not accounted for in this way. Furthermore, if the nuclei are produced in the case of chlorine by the reaction previously referred to, then they are presumably produced in bromine and iodine by the same type of reaction. It is well known that of the three reactions

$$2Cl_2+2H_2O=4HCl+O_2 \quad \ldots\ldots\ldots\ldots \quad (1)$$
$$2Br_2+2H_2O=4HBr+O_2 \quad \ldots\ldots\ldots\ldots \quad (2)$$
$$2I_2+2H_2O=4HI+O_2 \quad \ldots\ldots\ldots\ldots \quad (3)$$

(1) goes almost entirely to the right before attaining equilibrium; (2) does not proceed so far, whilst (3) hardly goes at all in this direction. It is difficult to see how this difference in the reactions can be reconciled with the observation that the nuclei in the present experiments are produced with equal readiness and abundance in all three cases.

Further work may show that this parallel is without foundation; but it is worthy of notice.

The writer wishes to express his very great indebtedness to Professor G. Owen for inspiration, advice, and encouragement during the course of this work.

XXVI.—THE CHARACTERISTIC CURVES OF LIQUID JETS.

By E. TYLER, *M.Sc.*, and E. G. RICHARDSON, *B.A.*, *M.Sc.*, *Ph.D.* (University College, London).

Received May 7, 1925.

ABSTRACT.

Continuing the work of S. W. J. Smith and H. Moss upon the relation between the length of a capillary jet and its velocity of efflux from a cylindrical orifice, further examination has been made of the causes to which the main features of the curves obtained by these authors are due. Such curves consist of two main branches. In the first, with increasing velocity, the jet length rises until a critical point is reached. In the second, which begins at this point, the jet length diminishes rapidly with further increase of velocity.

The results contained in the present Paper indicate that, while surface tension is of prime importance in the first parts of these curves, viscosity is the dominating factor in the second.

A general dimensional formula which covers both cases is derived.

INTRODUCTORY.

THE object of this research was to investigate further the behaviour of liquids issuing from a capillary tube under varying experimental conditions. Curves showing the relation between the "jet-length" (continuous portion of the cylindrical column of liquid) and "velocity of efflux" had previously been obtained by Prof. S. W. J. Smith and H. Moss,* and it was suggested to one of us (E. T.) to investigate the phenomena further. The work was commenced (Part I) in the Physical Laboratory at the University of Birmingham, under the supervision of Prof. Smith, and was later continued by us (Part II) in the Physical Laboratory at University College, London.

PART I.

The apparatus for producing the jets consisted of a metal cylinder, supported in a vertical position, provided with a water-gauge, and terminating in two axial tubes. To the lower of these the nozzle was attached by a short wired-on rubber tube. The upper outlet of the cylinder led to a reservoir, into which air could be pumped through a valve. The procedure in taking a set of readings was to fill the cylinder with the liquid, attach the nozzle, and gradually increase the pressure, registered on a mercury manometer, step by step by means of the pump. The air reservoir kept the pressure constant on the liquid, while readings of the jet-length L (by cathetometer) and the velocity of efflux V (by the volume of issuing liquid collected in a minute) were taken. The jet was illuminated by light from behind and from one side, the "end of the continuous portion" being taken as that point at which the two vertical bands of light reflected in the jet appeared to break up into diffuse light throughout the section of the jet.

The glass nozzles were constructed by drawing out capillary tubing (1 mm. bore), care being taken that the orifice at the point where the tube was subsequently cut was circular. The wax nozzles mentioned later were made by placing a capillary rod in a glass tube about ½ cm. in diameter, slightly coned at the lower end to prevent the wax being forced out by pressure, the interspace being then filled with molten paraffin-wax. The rod was withdrawn when the latter had set.

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* Proc. Roy. Soc., A. 93, 373 (1917).

Typical " L/V curves " are shown in Figs. 1 and 2. They consist of a short curved line, followed by a " kink " (at the " lower critical point "—Smith and Moss); and a straight line at less but still positive slope, until at another stage (the " upper

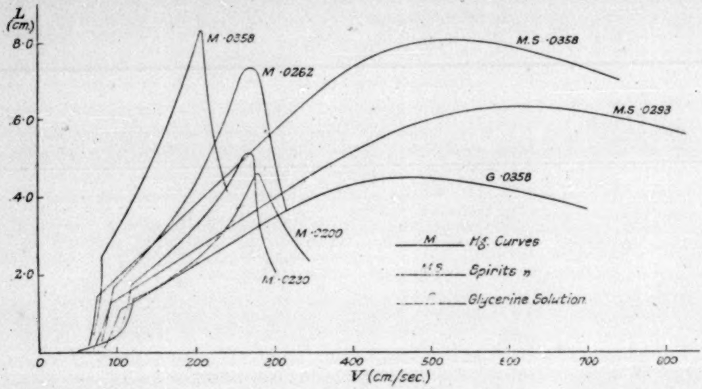

FIG. 1.

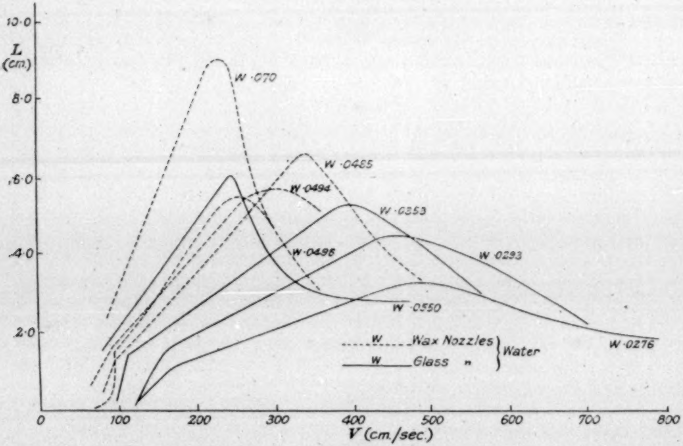

FIG. 2.

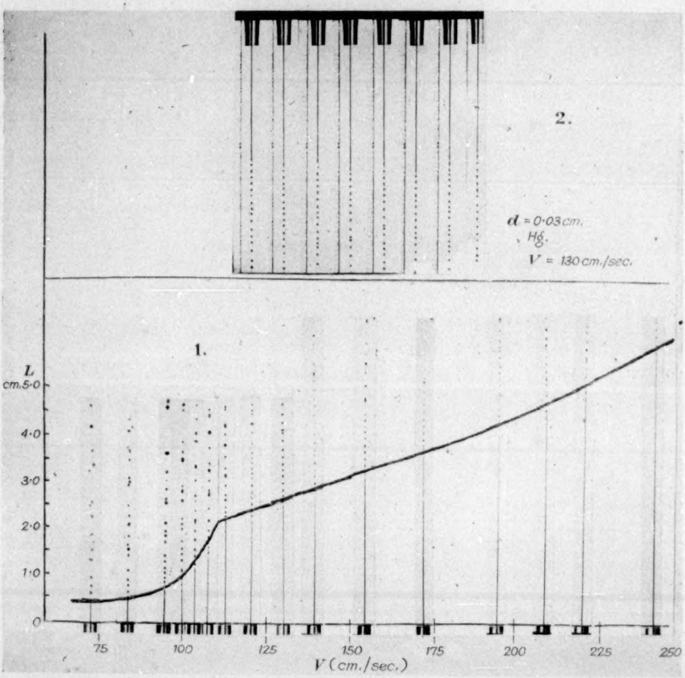

Plate 1 and 2.

[*To face page* **299.**

critical point "), the slope suddenly becomes negative, L falling in a curve of hyperbolic shape to a nearly constant value, and never actually reaching zero. The two principal parts of the curve—i.e., lower to upper critical points—beyond the upper critical point—will be distinguished as positive and negative parts respectively.

Photographs of the mercury jets were taken in order to check the jet-length as measured by the cathetometer. Instantaneous photographs were taken, using the spark discharge of a Leyden jar as illuminant. Some of the jet photographs, all obtained with the same nozzle ($d=0{\cdot}030$ cm., slightly convergent), are shown on Plate 1, placed in appropriate positions on the $L : V$ curve for the same nozzle, which had previously been obtained using the cathetometer to measure L. Having regard to the fluctuations which are always present—cf. Plate 2, which exhibits a series of photographs taken at constant V—it can be seen that the lengths as obtained by the two methods are in substantial agreement. The error in measurement of L is certainly not greater than that involved in V.

Similar curves were obtained by Smith and Moss, who proceeded to develop a theory of Lord Rayleigh, which will be discussed later, and obtained a formula for the positive part of the curve, by which they were able to reduce these positive parts to a single straight line (except in the case of mercury), and to give an interpretation of the linear relationship obtained. If we regard this linear relationship as an experimental fact, the formula (1) below can be deduced from dimensional principles.

Reduction Formula for the Positive Part.—Ignoring for the present the influence of viscosity, L may depend on the diameter of the nozzle (d), the density of the liquid (ρ), its surface tension (σ), and, of course, on V.

Let
$$L=\kappa\rho^x d^y \sigma^w V^n$$

Equating exponents of length, mass and time, we get
$$1=-3x+y+n$$
$$0=-2w-n$$
$$0=x+w$$

whence
$$L=\kappa\rho^{\frac{n}{2}} d^{1+\frac{n}{2}} \sigma^{-\frac{n}{2}} V^n$$

But experiment shows that L is proportional to V for a given jet ; therefore
$$L=\kappa\,\rho^{\frac{1}{2}} d^{\frac{3}{2}} \sigma^{-\frac{1}{2}} V$$

or
$$L/d=\text{constant}\times V\sqrt{\frac{\rho d}{\sigma}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \qquad (1)$$

The authors cited obtained only a few curves to test this relation, and the curves in Figs. 1 and 2 were obtained in order to test equation (1) further. In Fig. 3, instead of L against V simply, L/d is plotted against $V\sqrt{\frac{\rho d}{\sigma}}$.

Figure 3, page 300, shows that, by means of this transformation, following Smith and Moss, it is possible to represent the positive parts of all the curves by one straight line, which, if produced, would pass through the origin, with the aforementioned exception of mercury. It was thought that this discrepancy might be due to the fact that mercury, unlike water, does not wet glass, and therefore some wax nozzles were made. The results for these nozzles are shown on Fig. 2 by dotted lines, and with the above transformation on Fig. 3. The fact that the positive parts for wax nozzles lie approximately on the glass nozzle curves show that this explanation—the wetting effect—is not valid.

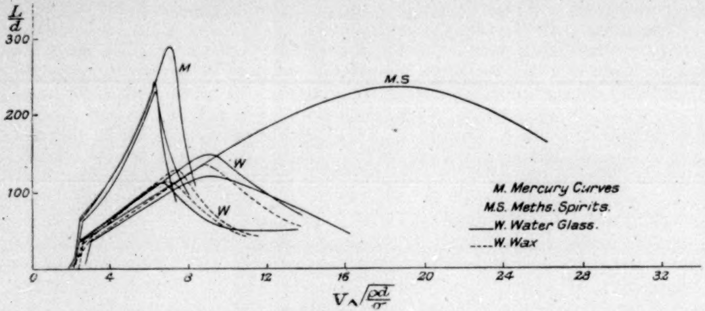

FIG. 3.

The Critical Velocities.—The values of the two critical velocities V_1 and V_2, and also $V_1\sqrt{\frac{\rho d}{\sigma}}$ and $V_2\sqrt{\frac{\rho d}{\sigma}}$, are shown in the following Table A for the liquids water, methylated spirit, and mercury.

TABLE A.

Liquid.	d cm.	Temp. ° C.	σ dynes / cm.	ρ gm. / cm.^{3}	V_1 cm. / sec.	V_2 cm. / sec.	$V_1\sqrt{\frac{\rho d}{\sigma}}$	$V_2\sqrt{\frac{\rho d}{\sigma}}$	$\frac{\mu}{\sqrt{\sigma\rho d}} \times 10^4$
Water (Glass Nozzle)	0·0550	20	72·3	1·00	...	246	...	6·8	50·0
	0·0358	13	74·0	1·00	110	400	2.38	8·7	73·6
	0·0293	15	74·0	1·00	155	455	3·00	8·8	77·3
	0·0276	15	70·5	1·00	160	490	3·16	9·7	81·9
	*0·02325	(17)	74·0	1·00	160	525	2·84	9·3	82·7
Water (Wax Nozzle)	0·070	20	72·3	1·00	...	235	...	7·3	44·2
	0·0496	20	72·3	1·00	95	260	2·50	6·8	52·7
	0·0494	15	74·0	1·00	98	310	2·53	8·0	59·6
	0·0485	16	72·3	1·00	100	340	2·60	8·8	59·8
Mercury (Glass Nozzle)	0·0358	15	547†	13·5	85	210	2·53	6·25	...
	0·0262	15	547	13·5	94	275	2·30	6·87	...
	0·0230	15	547	13·5	117	270	2·81	6·46	...
	0·0200	15	547	13·5	117	282	2·60	6·28	...
	*0·02325	(17)	547	13·5	125	405	3·00	9·70	...
Meth. Spirit (Glass Nozzle)	0·0358	15	24·9	0·83	75	550	2·36	19·0	213
	0·0293	17	24·9	0·83	95	625	2·71	19·5	223
	*0·02325	(13)	24·4	0·82	100	800	2·80	22·4	251

* Smith and Moss values.

† Assumed value from Quincke: Ann. der Phys., 52, 1 (1894).

Comparison with a similar table given by Smith and Moss (loc. cit.) shows that V_1 and V_2 are generally lower than those obtained in their research. This was attributed to the relative convergence of the nozzles used, and a series of experiments were now made, at Prof. Smith's suggestion, to test this point, using mercury and a single glass nozzle of the section shown in the next Figure (4), cut at successive points so as to vary the " taper " of the bore at the orifice.

Analysing these results, one sees that for a perfectly cylindrical nozzle—e.g., $d=0{\cdot}0246$ cm.—the value of $V_2\sqrt{\frac{\rho d}{\sigma}}$ is smaller than for one of greater convergency, whereas increase in the divergency (trumpet shape) lowers this value. Hence, the absolute value of this function $V_2\sqrt{\frac{\rho d}{\sigma}}$ will depend on the method of manufacture of the nozzles, and the discrepancy between these results and those obtained in the earlier Paper is explainable from this point of view.

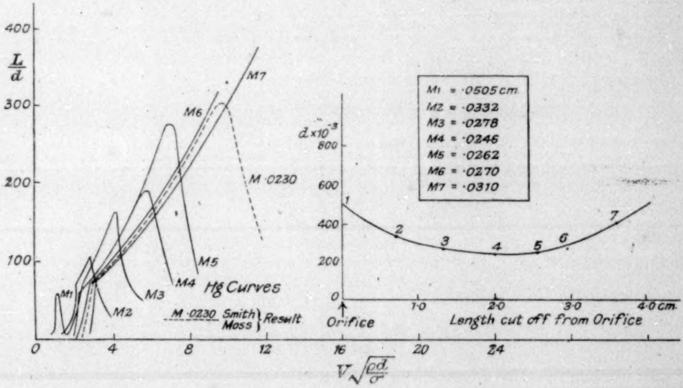

FIG. 4.

Effect of Viscosity on Upper Critical Velocity.—Referring to the results for spirit in Fig. 3, it will be seen that the slope of the positive part of the curve is the same as for water, except that the upper critical point is higher ($V_2\sqrt{\frac{\rho d}{\sigma}}$ is about 19 for spirit against 9 for water). The higher value was attributed* to the higher viscosity of methylated spirit, which is 61 per cent. greater than that of water. It was therefore of interest to test the effect of viscosity on the value of $V_2\sqrt{\frac{\rho d}{\sigma}}$ at the upper critical point, and solutions of glycerine and water were employed next (their surface tensions were approximately the same as that of water). One typical $L : V$

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* Cf. Smith and Moss, loc. cit., p. 386.

curve is shown in Fig. 1 (d=0·0358 cm.), the viscosity of the liquid being 94 per cent. greater than that of water. Further results are shown in Table B, with corresponding values of $\frac{\mu}{\sqrt{\sigma\rho d}}$.

TABLE B.—*Viscosity Results* (Water-Glycerine Solutions).

d cm.	Temp. °C.	$\frac{\sigma \text{ dynes}}{\text{cm.}}$	$\frac{\rho \text{ gm.}}{\text{cm.}^3}$	$\frac{V_1 \text{ cm.}}{\text{sec.}}$	$\frac{V_2 \text{ cm.}}{\text{sec.}}$	$V_1\sqrt{\frac{\rho d}{\sigma}}$	$V_2\sqrt{\frac{\rho d}{\sigma}}$	$\frac{\mu}{\sqrt{\sigma\rho d}}\times 10^4$
0·0358	20	71·2	1·06(4)	105	500	2·5	11·6	117·5
0·0358	20	71·8	1·04(8)	...	452	...	10·3	96·8
0·0358	20	74·3	1·03(5)	...	454	...	10·1	83·2
0·0358	20	68·9	1·02(3)	...	402	...	9·2	80·5
0·0358	18	75·5	1·01(4)	...	410	...	9·0	71·9
0·0358	13	74·0	1·00	...	400	...	8·7	73·6

Suggested Dimensional Theory to account for the Upper Critical Point.—Expressing V in terms of σ, ρ, d and μ, Smith and Moss* have obtained the following dimensional equation for the upper critical velocity,

$$V_2=\kappa\sigma^x\mu^{1-2x}(\rho d)^{x-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where x is an undetermined index, and κ is a constant.

Starting from this equation, we put $1-2x=n$ in (2), and derive

$$V_2=\kappa\sqrt{\frac{\sigma}{\rho d}}\left(\frac{\mu}{\sqrt{\sigma\rho d}}\right)^n$$

In this expression, $\sqrt{\frac{\sigma}{\rho d}}$ is of the dimensions of a velocity, whereas the other term is non-dimensional. Consequently, the general form is

$$V_2=\kappa\sqrt{\frac{\sigma}{\rho d}}\cdot f\left(\frac{\mu}{\sqrt{\sigma\rho d}}\right)$$

If now the function f be expanded as a linear series, the second and higher powers of $\frac{\mu}{\sqrt{\sigma\rho d}}$ may be neglected, since this quantity is small, so that the simplest possible form of the equation, giving the upper critical point, when the effects of viscosity are not negligible, is

$$V_2=\kappa\sqrt{\frac{\sigma}{\rho d}}\left\{a+\frac{b\mu}{\sqrt{\sigma\rho d}}\right\}$$

or

$$V_2\sqrt{\frac{\rho d}{\sigma}}=A_1+B_1\frac{\mu}{\sqrt{\sigma\rho d}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where A and B are constants.

Equation (3) is a straight line not passing through the origin. In Fig. 5 the two variables of (3) are plotted, taking the critical values from Tables A and B, and from the various $L:V$ curves in the figures. The graph agrees well with the theoretical forecast. *The upper critical velocity, therefore, involves both surface tension and viscosity.*

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* loc. cit., p. 376

PART II.

Viscosity Effect (*con.*).—The object now was to vary viscosity, keeping surface tension constant as far as possible, and therefore some sugar solutions were made of various concentrations, as as to produce liquids up to 300 per cent. more viscous than water. Curves for spirit and paraffin oil were also obtained (Figs. 6, 7, 8),

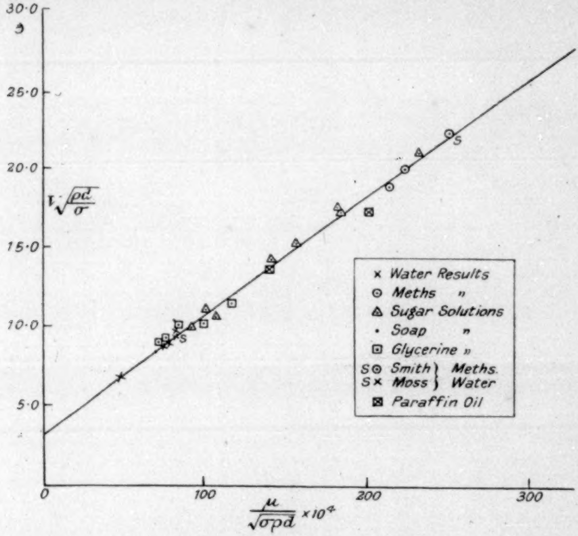

FIG. 5.

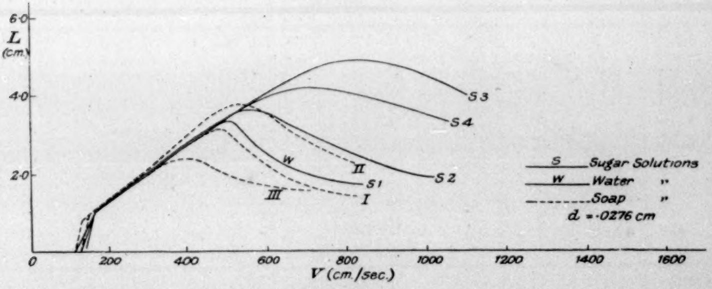

FIG. 6.

and, with a view to varying the surface tension without the viscosity, soap solutions were tried. But the positive slopes of the soap curves coincided nearly with the water curves, due, no doubt, to the surface tension of the freshly formed soap solution being nearly as high as that of water (vide Lord Rayleigh*).

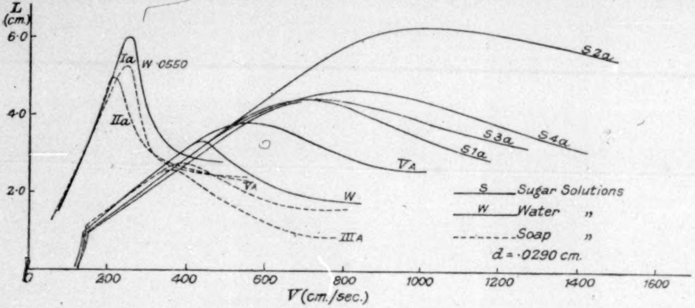

FIG. 7.

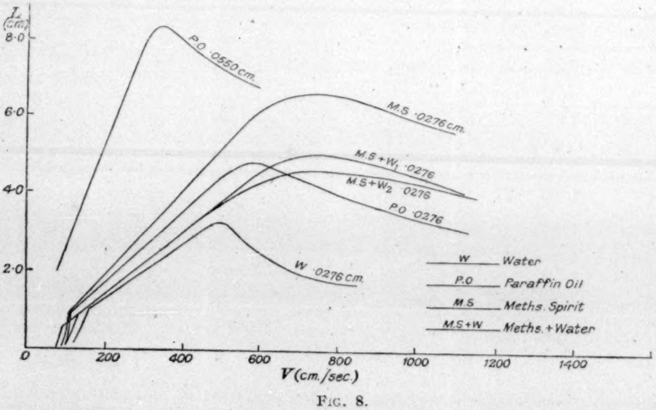

FIG. 8.

In confirmation of previous results, the sugar, soap and water positive parts practically coincide (σ unchanged), the upper critical points (both L and V) are raised by increasing the viscosity. The reduced curve for the whole series is shown in Fig. 9.

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* Proc. Roy. Soc., 47, 281 (1890).

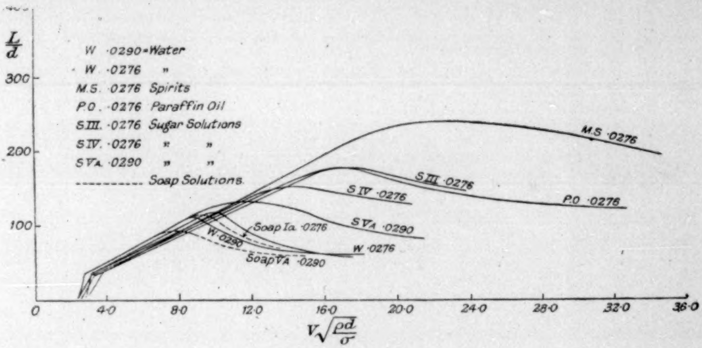

FIG. 9.

TABLE C.

Liquid.	d cm.	Temp. ° C.	σ dynes/cm.	ρ gm./cm.³	V_1 cm./sec.	V_2 cm./sec.	$V_1\sqrt{\frac{\rho d}{\sigma}}$	$V_2\sqrt{\frac{\rho d}{\sigma}}$	$\frac{\mu}{\sqrt{\sigma\rho d}} \times 10^4$
Water.	0·0290	17	72·3	1·00	139	437	2·79	8·77	74·7
	0·0276	15	70·5	1·00	160	490	3·16	9·7	81·9
Paraffin.	0·0550	20	26·4	0·804	...	340	...	13·9	140·5
	0·0276	20	26·4	0·804	110	590	3·19	17·15	199·0
Sugar Solution 1	0·0276	15	71·0	1·017	160	502	3·16	10·0	88·5
2	0·0276	18	71·7	1·055	162	549	3·20	10·9	107·8
3	0·0276	18	71·8	1·120	160	825	3·32	17·2	180·3
4	0·0276	16	72·7	1·085	160	711	3·25	14·4	141·0
1a	0·0290	17	73·9	1·090	140	710	2·90	14·70	141·0
2a	0·0290	14	73·3	1·135	150	1000	3·18	21·21	232·0
3a	0·0290	16	73·3	1·098	140	740	2·91	15·40	155·0
4a	0·0290	16	73·0	1·115	130	841	2·72	17·62	178·5
5a	0·0290	16	72·3	1·053	140	557	2·86	11·45	101·6
Meth. S.	0·0276	17	23·7	0·813	118	727	3·65	22·47	250·0
Meth. S.-Water.	0·0276	18	53·0	0·95	105	767	2·3	17·15	180·5
	0·0276	15	43·2	0·98	130	753	3·2	18·80	210·0
Soap Soln.	0·0550	19	...	0·99	...	250	...	6·9*	50·0*
	0·0276	15	...	0·99	...	470	...	9·3	81·9
	0·0290	15	...	0·99	140	400	...	8·32	69·8

*Assuming $\sigma = 73$ dynes/cm.

These results are included on Fig. 5.

SUGGESTED THEORY.

We have now to account for the following characteristics of the curves :—

(1) The positive part.

(2) The negative part.

(3) The upper critical point.

(4) The lower critical point, and the asymptotic tendency of the negative part when V is much increased.

Plateau* first attempted to explain the breaking-up of a cylindrical column of liquid, and showed that when a disturbance acted upon such a column it produced alternate swellings and contractions under the action of surface tension. If the "wave-length" of these appearances (λ) was greater than the circumference of the jet, they grew until finally the jet was split up into separate drops. Rayleigh,† adopting this theory, has shown that, when Plateau's criterion is satisfied, any disturbance impressed upon the liquid column at the orifice will produce waves, which will travel downwards, the amplitude of the disturbance growing exponentially until, finally, it will break up the jet. The rate of growth of this disturbance is most rapid when $\lambda=4{\cdot}51\times d$, and the time of disintegration in this case is given by

$$t=0{\cdot}696\sqrt{\frac{\rho d^3}{\sigma}}$$

Scheuerman,‡ working out the velocity of capillary waves, moving on a liquid jet. has obtained a formula identical with Rayleigh's ; and he supports the latter's theory of the cause of disintegration, as opposed to that of Buff,§ who supposed the swellings to be produced by the formation of drops at the end of the jet, the swellings in turn producing waves that travel *up* the jet. Smith and Moss point out that, with a given velocity of efflux V, the continuous length of the jet L is given by $L=Vt$ or, assuming the maximum instability case, by

$$L=0{\cdot}696V\sqrt{\frac{\rho d^3}{\sigma}}\log\frac{a}{a_0}$$

where a_0=initial amplitude of disturbance at the orifice and a=final amplitude of disturbance causing disintegration ; and that, in this event, the significance of the experimental result

$$\frac{L}{d}=\text{constant}\times V\sqrt{\frac{\rho d}{\sigma}}$$

is (l.c., p. 388) that the ratio a/a_0 is constant.

The Positive Part.—This, following Smith and Moss, we consider to be an effect of surface tension alone—i.e., apart from viscosity.

The Negative Part.—The shape of this part of the curve, and the turbulence which determines the continuous length of the jet, we consider to be due to viscosity, surface tension playing a very small part in producing it. The "end of the jet" here represents the point in the accelerating liquid, at which the velocity of adjacent filaments reaches that value, at which, with the given jet, the Osborne Reynolds' criterion for breakdown of the stream-line into turbulent motion—i.e., the stream velocity—has reached a definite value V at this point in the jet where the diameter is D. Increase of velocity of efflux will bring this point nearer the nozzle ; L would be expected to decrease with increasing V, as it does. In support of this idea, the position of the negative parts of the curves on Figs. 7 and 8 may be noted. With a given liquid, decrease of " d " increases all the velocities ; with a given jet, increase of " dynamical viscosity " also moves these parts bodily in the direction of greater V.

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* Statique des Liquides, p. 354 (1873).

† Sound, II, 360.

‡ Ann. der Phys., 60, 234 (1919).

§ Ann. de Chim., 78, 162 (1851).

In order to test this theory—i.e., that the negative part was a viscosity effect, we thought that if we could remove surface tension from the liquid, we should get the negative part alone, and that L would continually decrease as V was increased. In order to accomplish this, a jet of water coloured with Meldola's blue, which left its density and viscosity unchanged, was allowed to fall from a variable " head " into clear water, thus removing the surface between the jet and the air, although, of course, a discontinuity still existed between the incoming water and the still water in the tank. The results justified expectation ; at small heads, and in the absence of external disturbances, the blue liquid fell in an unbroken column to the bottom of the tank. On increasing V by raising the head, a turbulent point appeared in the jet near the bottom, rose quickly to within 4 cms. of the orifice, and then more gradually to ½ cm. with the greatest heads we could use. The $L : V$ curve for this submerged nozzle was thus hyberbolic in shape (Fig. 10) and lay with fairly good agreement, considering the rather different conditions, on the corresponding $L : V$ (negative) curves for the ordinary water-air jet, in the case of the two diameters (0·0550 and 0·0290 cm.) used. V in this new experiment had to be obtained from the head by the Torricellian formula $V=\kappa\sqrt{2gH}$, which is only approximate for these narrow jets. Furthermore, by taking the ratio of corresponding values of V which produced the same L; we found this ratio to be nearly constant, and the same as the inverse ratio of the respective diameters. That is, V is proportional to $1/d$, which would be expected from a Reynolds' relation $\frac{VD\rho}{\mu}$=constant. We take, therefore, for the equation of this part of the curve

$$\frac{Vd\rho}{\mu}=B, \text{ a constant at constant } L \text{ . . . . } \quad (4)$$

The shape of the $L : V$ curve can perhaps be explained by introducing L in the form $f(d/L)$, writing $V=B'\frac{\mu}{\rho d}f\left(\frac{d}{L}\right)$ and expanding $f\left(\frac{d}{L}\right)$ as a linear function

$$V=B_2\frac{\mu}{\rho d}\left(1+C\frac{d}{L}\right)$$

since L is large compared with d.

$$V=B_2\frac{\mu}{\rho d}+C_2\frac{\mu}{\rho L} \text{ . . . . } \quad \textbf{(4a)}$$

whence we see that, *ceteris paribus*, LV is a constant. This is a hyperbola like that actually obtained, and this equation can be used to reduce the negative parts of the curves to the same line just as (1) was used for the 1st part.

The Upper Critical Points.—Analysis of the proceeding results, given in Part I, indicates that the upper critical velocity is dependent on both surface tension and viscosity, and can be represented by equation (3). Further evidence of the truth of this equation is given by plotting on the same graph (Fig. 5) the additional critical values obtained in this second part from the last two columns of Table C. It will be seen that these results also lie on the straight line given by equation (3). This equation can now be written thus:

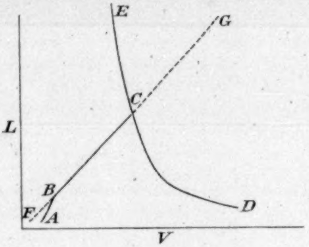

FIG. 10B.

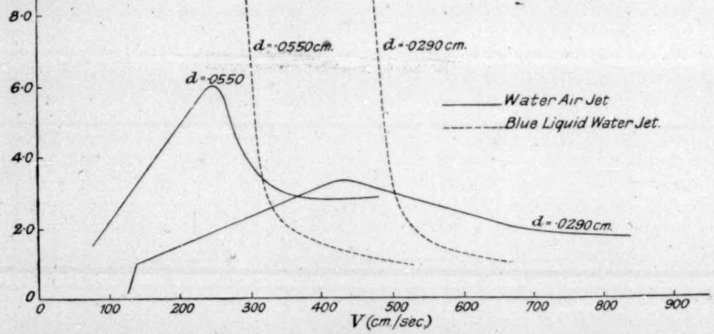

FIG. 10A.

$$V_2 = A_1\sqrt{\frac{\sigma}{\rho d}} + B_1\frac{\mu}{\rho d} \qquad \text{(3a)}$$

and this form of the equation suggests a combination of equations (1) and (4). In order to illustrate this point further, we introduce the function $f(d/L)$ thus

$$V = A_2 f_1\left(\frac{L}{d}\right)\sqrt{\frac{\sigma}{\rho d}} + B_2 f_2\left(\frac{d}{L}\right)\frac{\mu}{\rho d} \qquad \text{(5)}$$

If, now, viscosity has no influence, B_2 must vanish, and

$$V = A_2 f_1\left(\frac{L}{d}\right)\sqrt{\frac{\sigma}{\rho d}}$$

putting $f_1\left(\frac{L}{d}\right)=\frac{L}{d}$ simply we get equation (1) of the positive part of the curve (in Fig. 5 the intercept on the "y" axis represents A_2L/d).

On the other hand, if the influence of surface tension is negligible, (5) becomes, putting $A_2=0$,

$$V=B_2f_2\left(\frac{d}{L}\right)\frac{\mu}{\rho d}$$

which is equation (4) of the negative part. Equation (5) is then the *general equation* of the whole curve, excepting the small part below the lower critical point.

Anomalies when the Jet-length is Small.—Measurements of photographs of jets show that there is a sudden contraction at the orifice as the liquid (mercury) leaves the nozzle. This fact—and in addition the observation that, when the velocity of efflux is small, the continuous portion of the jet is traversed by ripples—can perhaps explain the abnormally low values of L when V is small—i.e., before the lower critical velocity V_1 is reached. These ripples, reacting on the nozzle, add an additional source of disturbance, tending to break up the jet sooner than if they were unable to reach the nozzle. A further cause of irregularity is to be looked for in the drop formation taking place very close to the nozzle ; this may also cause a reaction at the nozzle tending to bring about disintegration in an unusually short time.

At higher velocities, on the negative part, a distinct " neck " was formed just outside the nozzle, and the resulting convergence of the stream between the nozzle and the " neck " no doubt prevents turbulence reaching the nozzle, even at the highest heads employed ; L never again becomes zero. This reminded us of the similar experience of Osborne Reynolds in his experiment.

SUMMARY.

The general phenomenon consists, therefore, of three distinct effects, the $L:V$ curves consist of three characteristic curves or phases superimposed :

(1) A straight line relation between L and V, the disruption involving surface tension effects, except at small lengths, when

(2) L/V is increasing due to drop-formation near the jet, and

(3) a hyperbolic relation between L and V, the breaking-up being due to turbulence caused by viscosity.

Elimination of either surface tension or viscosity would leave effect (3) or (1) respectively alone. The former has been practically eliminated in the experiment described above. To eliminate the viscosity effect would require a liquid possessing properties which no actual fluid has, falling freely into air, so that such conditions are unattainable. Again, the elimination of the initial phenomena at the jet should make (1) continue as a straight line to the origin.

These theories are summarised in Fig. 10B, which incorporates a $L:V$ curve $ABCD$ with the corresponding observed continuation into the surfaceless phase CE, together with the theoretical continuation of the pure surface tension curve $FBCG$. Both surface tension and viscosity would effect disruption at a definite point in the jet. The actual length of a jet measured in air is determined by that one of these two properties which produces its effect first. At the upper critical point they would, operating alone, each break up the jet at the same place.

We are much indebted to Professor S. W. J. Smith, F.R.S., of Birmingham, both for his help in the work described in Part I and also for kind assistance in compiling the Paper. We are also grateful to Professor A. W. Porter, F.R.S., for kindly interest and criticism during the latter part of this work.

DISCUSSION.

Mr. W. E. Benton (communicated): With reference to this very interesting Paper, I should like to point out that some slight obscurity results from the use of dimensional equations in the way which the authors describe. Equation (3) cannot possibly be derived from equation (2). Professor S. W. J. Smith, in his original Paper, suggested that the exponent "x" may, in itself, be of interest. From the experimental fact that, in the cases examined, "x" appeared to approximate to the value 0·5, he concluded that in these the influence of viscosity was at most very small. In the present work, the authors have assumed that the effect of viscosity is small, and on that assumption have written down equation (3), which can only be an expression of the possibility that the ratio of V_2 to the quantity $\sqrt{\frac{\sigma}{\rho d}}$ (which is also a velocity) is of the form $\left(A+B\frac{\mu}{\sqrt{\sigma\rho d}}\right)$. This involves the further assumption that μ only appears in the equation as part of a non-dimensional parameter. Both assumptions, however, appear to be justified by the experiments. It might further be suggested that there seems no justification for the broad assumption contained in the equation

$$L=K\rho^{x}d^{y}\sigma^{w}V^{n} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{I})$$

Since $\sqrt{\frac{\sigma}{\rho d}}$ is of the nature of a velocity, it is possible that experimental evidence would support an equation of the form

$$\frac{l}{d}=Kf\left(V\sqrt{\frac{\rho d}{\sigma}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{II})$$

Equation (1), however, gives

$$\frac{l}{d}=K\left(V\sqrt{\frac{\rho d}{\sigma}}\right)^{n}$$

which is only a particular case of (II).

It appears from Fig. 3 that their equation (I) is only true for the methylated spirit curve. The corresponding portions of the water curves, when produced, do not pass through the origin. The curves for mercury appear to be parabolic.

Prof. S. W. J. Smith (communicated): I rather regret that more of the excellent photographs of the jets obtained by Mr. Tyler, when a student in Birmingham, were not submitted for reproduction on a larger scale than those of the Plate which the Paper contains.

I had hoped also that Mr. Tyler would be able to include in the Paper some of the interesting results which he obtained, when working in my laboratory, on the influence of a magnetic field upon the form of the "characteristic curves." These experiments were begun by myself and Mr. Moss in the hope that they would throw further light on the significance of the results obtained below the lower "critical velocity," but circumstances did not permit us to complete them.

Mr. R. S. Maxwell (communicated): The results obtained for mercury show a marked dissimilarity from those for the other liquids, particularly in what is termed the "positive" part of the curve. That this part is distinctly non-linear may be seen in Figs. 1 and 3. A possible explanation of this discrepancy may lie in the fact that mercury has a density of about thirteen times that of any of the other liquids used, whose densities are very nearly that of water. It would be interesting if experiments could be made on a liquid whose density is about half that of mercury for this would afford a check on the theory given. As there are not many suitable liquids, it would probably be necessary to work with molten metals, though in this case the expermental difficulties would be considerable. Fig. 4 shows that the position of the upper critical point depends upon the convergency or divergency of the nozzle. In Fig. 5 the results of a large number of observations, on different liquids, and by different observers, are collected together, and the points are shown to lie approximately on a straight line. Were any precautions taken to ensure that cylindrical nozzles were used in all these experiments, as otherwise the positions of the critical points would be inaccurate?

AUTHORS' reply: We are very grateful for the remarks of Mr. Benton pointing out the importance of the assumptions necessary in obtaining the dimensional equation 3 in the Paper.

We see no reason why, however, equation 3 cannot be derived from equation 2 when a knowledge of n is available, or, moreover, when μ appears in the equation as part of a non-dimensiona parameter.

His remarks on the non-dimensional quantity $\frac{\mu}{\sqrt{\sigma \rho d}}$ do not appear quite clear to us, unless he means that you cannot go back from $\left(\frac{\mu}{\sqrt{\sigma \rho d}}\right)^n$ to $f\left(\frac{\mu}{\sqrt{\sigma \rho d}}\right)$ unless μ only occurs in this non-dimensional expression, and in that he is right. The derivation of equation (II) given by Mr. Benton we regard as a process generally followed in evolving dimensional equations, that is from $f\left(V\sqrt{\frac{\rho d}{\sigma}}\right)$ to $\left(V\sqrt{\frac{\rho d}{\sigma}}\right)^n$, and it was not our intention to exhibit every step of the mathematical process.

Within experimental error the water curves in Fig. 3 do pass through the origin. The spirit curve is curtailed only because we could not use higher pressures, otherwise it is just like the water curves.

In reply to Prof. S. W. J. Smith's communication, it is hoped to publish shortly some interesting results on the influence of a magnetic field upon the form of the "characteristic curves."

The remarks of Mr. Maxwell referring to the discrepancy of the results between mercury and the other liquids, and attributing same to the excessive density of the former, are in agreement with the Authors' views. We have attributed the "positive part" of the LV curves to be an effect of surface tension alone—i.e., apart from viscosity. Hence, assuming any disintegrating disturbance at the nozzle capable of breaking up the jet in a constant time t, since observation shows that the diameter of the jet is uniform down to within a few mms., where it breaks up, the velocity remains constant down the jet.

So that if t=time of disintegration of the disturbance, which is carried down the jet with velocity V, the jet will break up after a length L, given by $V=L/t$—i.e., the $L:V$ graph is a straight line, or $V \alpha L$.

Now, in the case of mercury, we suggest excessive density causes a continuous change of diameter down the jet, showing that V increases down the jet, so that L/V (V is now the velocity at the nozzle only) tends to increase as V increases. The order of "taper" required, however, does not appear on the photographs. It would indeed be of extreme interest to carry out further experiments using molten metals, and we are very grateful for this suggestion.

Mr. Maxwell quite rightly emphasises the importance of taking precautions to ensure that cylindrical nozzles were used, and to accomplish this condition the capillary tubing on being drawn out was cut at the point considered to give cylindricality (generally midway between points at which tapering began) and both halves of the tubing, each forming a nozzle, were experimented with and if the $L:V$ curves for both parts coincided, then this was sufficient to ensure that the nozzles were cylindrical.

XXVII.—AN INVESTIGATION OF THE CONTROL CONDITIONS UNDER WHICH NEWTON'S LAW IS VALID FOR THE EMISSION OF HEAT FROM ELECTRICALLY HEATED WIRES.

By Sybil Marshall, *A.R.C.S.*, and J. O. C. Vick, *A.R.C.S.* (Imperial College of Science and Technology, London).

Received May 21, 1925.

(*Communicated by* H. S. Gregory, Ph.D., D.I.C.)

Abstract.

The following Paper deals with the variation with temperature excess of the controlled heat loss from an electrically heated wire. The wire, whose temperature was measured by means of a Callendar-Griffiths bridge, was surrounded by air at a known pressure enclosed in a glass tube, the latter being immersed in a constant temperature bath. The rate of heat loss, therefore, could be calculated from a knowledge of the current used and the resistance per unit length of the wire. Total heat loss/temperature excess curves were plotted, these tending to become straight lines as the convection was reduced.

I. Introduction.

A PRECISE knowledge of the conditions under which Newton's Law is valid is essential in the determination of the thermal conductivity of metals, as may be seen from the following equation representing the thermal equilibrium of an element δx of an electrically heated wire surrounded by a gaseous atmosphere

$$KA\frac{d^2\theta}{dx^2}\,.\,\delta x+\frac{C^2\rho_0(1+\alpha\theta)\,\delta x}{JA}=f(N,E,p,\theta)s\,.\,\delta x\,.\qquad(1)$$

where K=thermal conductivity of the wire,
θ=temperature excess of element of wire above the surrounding atmosphere,
A=cross-sectional area of the wire,
C=current passing in the wire,
ρ_0=specific resistivity of the wire at 0°C.,
α=coefficient of increase of resistance with temperature of the wire,
δx=elementary length of wire,
s=perimeter of wire,
J=mechanical equivalent of heat.

The expression on the right-hand side of the equation, $f(N, E, p, \theta)s\,.\,\delta x$, represents the loss of heat from the surface of the element, which in general depends on the nature, extent, pressure and temperature of the surrounding medium. Many investigators have, in order to simplify the theoretical aspect of equation (1), assumed

Newton's Law of cooling, by which the loss of heat from the element considered is proportional to its excess of temperature above that of the surrounding medium, i.e.,

$$f(N, E, p, \theta)s \,.\, \delta x = \lambda\theta s \,.\, \delta x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

where λ is some constant.

A theoretical treatment of the cooling of bodies by both free and forced convection has been worked out by Oberbeck[*], Boussinesq[†], Lorenz[‡] and Russell[§], who have shown that Newton's Law is strictly true only for forced convection. Experimental observations have been made by Dulong and Petit[‖], Macfarlane[¶], Nichol[**], Brush[††] and others, on the rate of cooling of bodies of known specific heat, in order to determine their emissivity ; whilst Schliermacher[‡‡], Ayrton and Kilgour[§§], Goldschmidt[‖‖], Kennelly[¶¶], Langmuir[*†], King[*‡] and others have obtained the emissivity by observing the loss of electrical energy from wires. These results confirm the predictions of theory.

The experiments described in the present Paper were undertaken in order to determine how far the emission of heat from electrically-heated wires could be controlled so as to conform to Newton's Law. The term "controlled emission" is here used to denote the heat loss from an electrically-heated wire, coaxial with a cylindrical tube whose external surface is maintained at a constant temperature (0°C.), the space between the wire and the wall of the tube being occupied by a gaseous medium.

In the case under consideration, if the temperature of the wire is uniform throughout its length, then $\dfrac{d^2\theta}{dx^2}$ is zero, and equation (1) may be written:

$$\frac{C^2R_\theta/l}{J} = \frac{2\pi K_g\theta}{\log_e r_2/r_1} + \varphi(\theta, p, r_1, r_2) + \psi(\theta) \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

where C=current in the wire,
R=resistance of length l of wire at uniform temperature θ,
J=mechanical equivalent of heat,
K_g=thermal conductivity of gas surrounding the wire,
θ=excess temperature of wire above inner walls of tube,
r_1=radius of wire,
r_2=internal radius of tube.

The first term on the right-hand side represents the heat loss by conduction through the gas, from the wire to the walls of the tube, according to Fourier's Law. Maxwell has shown theoretically on the Kinetic Theory that this is independent of pressure down to a fairly low pressure, depending on the thickness of the gas layer, and recently this has been shown to hold experimentally by Gregory and Archer ; the controlling factors in this expression then are the temperature excess of the wire above the wall of the tube, and the dimensions of the wire and tube.

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[*] Ann. Phys., 1879, VII, 271.
[†] Comp. Rend., Vol. 133, p. 497.
[‡] Ann. de Physik, 1881, XIII, 422, 582.
[§] Phil. Mag., 1910, XX, 591.
[‖] Ann. de Chim. et de Phys., 1817, VII.
[¶] Proc. Roy. Soc., 1872, XV, 90.
[**] Proc. Roy. Soc. Edin., 1869, VII, 206.
[††] Phil. Mag., 1898, XLV, 31.
[‡‡] Wied. Ann., 1885, XXVI, 287.
[§§] Phil. Trans., 1892, CLXXXIII, 374
[‖‖] Elek. Tech. Zeit., 1908, XXIX, 886.
[¶¶] Trans. Amer. I.E.E., 1909, XXVIII, 363.
[*†] Physical Review, 1912, XXXIV, 401.
[*‡] Trans. Roy. Soc., 1914, CCXIV, 373.

The function $\varphi(\theta, p, r_1, r_2)$ denotes the heat losses by convection from the wire, which depend largely upon the pressure of the gas and the dimensions of the tubes, and to a less extent on the temperature gradient.

The function $\psi(\theta)$ denotes the heat loss by radiation from the wire, which depends only on the temperature difference between the wire and the walls, and not at all on the pressure or dimensions of the apparatus.

Thus at constant pressure equation (3) may be written:

$$\frac{C^2R_\theta/l}{J}=f(\theta) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The aim of this investigation is to ascertain in what way the total emission depends upon the control conditions, and how these conditions can be fixed precisely so as to make $f(\theta)$ in equation (4) conform to Newton's Law.

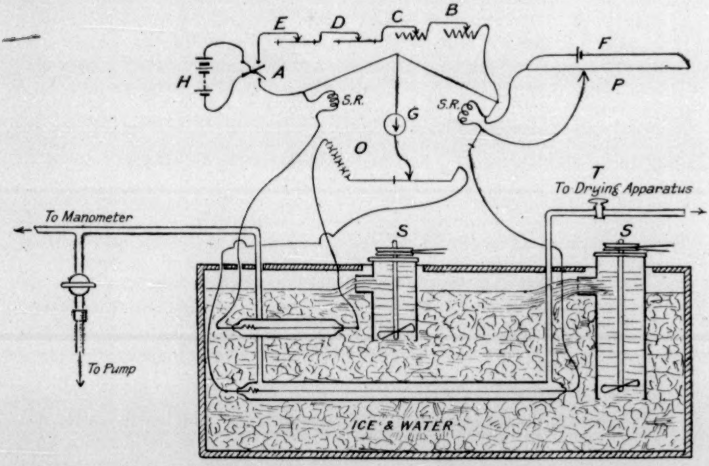

Fig. 1.—Apparatus and Connections.

II. Apparatus.

The apparatus was that used by Gregory and Archer in their determination of the thermal conductivity of air and hydrogen.

A diagram of the apparatus and connections is shown in Fig. 1. A long and a short tube of the same even bore were mounted horizontally and in gaseous connection. Down the axis of each tube was stretched a length of 6 mil platinum wire, to the ends of which were soldered leading wires of thick platinum sealed into the tubes. One leading wire in each tube, in the form of a spiral spring, was slightly stretched in mounting, so that any extension of the wire on passing a current through it would be taken up, thus maintaining the wire coaxial with the tube at all temperatures. The two wires were placed in the main and compensating arms of the Callendar-Griffiths bridge O. The shorter tube or compensator was so arranged that it compensated for the heating effect in the leading wires, the loss of heat along the leading wires, and the thermo-electric effects produced in the main tube, in effect, isolating a length *l* of the wire, which was found from experiment to be equal to the difference in length of the two wires.

The mathematical analysis of this compensation is of too lengthy a nature to be here quoted, but is in course of publication by Gregory and Archer, to whom it is due.

A standard resistance SR of one ohm was placed in each of the opposing arms of the bridge, and the potential difference across one of these resistances was measured by means of the potentiometer *P* (Cambridge Scientific Instrument Co.'s "slide" type), *F* being a fairly large capacity accumulator. The current, supplied by the battery *H*, consisting of two sets of six 2-volt accumulators in parallel, entered the bridge, having passed through the commutator *A* and the sliding rheostats *B*, *C*, *D*, *E*, of which *B* and *C* were of the usual type, whilst *D* and *E* were stretched pieces of thick and thin manganin wire with sliders attached. By this means a very fine adjustment of current strength could be obtained. A moving-coil galvanometer *G* of a very sensitive type was used in the bridge circuit.

The tubes were immersed in a bath containing water, melting ice and snow, which were kept circulating and well stirred by means of *S*, *S*, two motor-driven stirrers. A Gaede pump was used to exhaust the tubes, whilst air, after having been passed over drying apparatus containing phosphorus pentoxide, could be admitted to the tubes through the tap *T*. A cathetometer microscope was used to measure the pressure as indicated by a mercury manometer.

The sliding contact on the Callendar-Griffiths bridge was fitted with a vernier, enabling readings on the bridge wire to be taken correct to 0·01 cms., so that since the resistance of the bridge wire was about 0·01 ohms per cm., readings could be taken correct to 0·0001 ohms, the fundamental interval being approximately 2·05 ohms.

The scale on the slide wire of the potentiometer was subdivided so that readings corresponding to differences of 0·0001 volts could be taken. Since these readings were of the potential difference across a standard 1 ohm coil, the readings of the potentiometer showed directly the current in amperes flowing through the coil and the wires in the tubes.

Two sets of tubes as described above were used, of radius 0·4592 cms. and 1·1714 cms., to show the effect of the alteration of dimensions of the tubes on the emissivity.

III. EXPERIMENTAL PROCEDURE.

The coils of the two bridges used were first carefully calibrated in terms of the coils of highest resistance. The resistance of two measured lengths, each about a metre, of the platinum wire was obtained, immersed first in ice and then in steam. Pieces cut from these wires were then mounted in the tubes as already described.

A linear graph could then be drawn connecting the resistance per unit length of the wire with its temperature, from which the resistance for any temperature θ could be read off.

The above preliminary observations being taken, the apparatus was then connected up as shown in Fig. 1.

The bridge contact was set so as to correspond with a prearranged temperature, calculated from a knowledge of the fundamental interval for the tubes. and, having switched on the current, by adjustment of the resistances B, C, D, E, the current through the bridge and tubes was varied until the bridge was balanced, thus indicating that the wires in the tubes were at the desired temperature. Immediately, a potentiometer reading was taken to determine the current flowing in the circuit, and then another reading of the bridge galvanometer to make sure no change of current had taken place in the main circuit since last giving a balance.

Other settings of the bridge were taken, corresponding to various temperature gradients between 5° and 50°, necessitating currents of from 0·2 to 0·5 amperes. Series of readings were obtained at constant pressure, for pressures varying from 760 mms. to 60 mms., first with the narrow and then with the wide tubes.

IV. RESULTS.

Dimensions.	*Narrow.*	*Wide.*
Internal radius of the tube	0·4592 cms.	1·1714 cms.
External radius of the tube	0·5215 cms.	1·2360 cms.
Radius of the platinum wire	0·007696 cms.	0·007696 cms.

The results obtained are tabulated below, the tables being made up as follows:—

Column (1) Series number and pressure in cms. of mercury at which the readings were taken.

Column (2) The temperature θ on the platinum scale as calculated from the bridge reading.

Column (3) The resistance per unit length in ohms per cm. at temperature θ as obtained from a knowledge of the fundamental interval.

Column (4) The current C in amperes as deduced from the potentiometer reading.

Column (5) Heat loss in watts per unit length of wire.

The readings for the wide tubes were taken with the bridge used for the narrow tubes for purposes of comparison.

NARROW TUBES.

(1) Series and Pressure	(2) Temperature θ	(3) R_θ/l	(4) Current C	(5) $\frac{C^2R_\theta}{l}$
I 75·7	40·3	0·06134	0·5137	0·01620
	36·6	0·06060	0·4891	0·01448
	32·9	0·05985	0·4645	0·01293
	29·6	0·05915	0·4382	0·01135
	25·4	0·05825	0·4103	0·009825
	21·65	0·05732	0·3788	0·008240
	17·78	0·05670	0·3453	0·006775
	14·14	0·05597	0·3079	0·005315
	10·40	0·05520	0·2629	0·003818
II 66·1	45·95	0·06250	0·5438	0·01812
	42·2	0·06170	0·5204	0·01679
	36·6	0·06060	0·4866	0·01436
	32·9	0·05985	0·4623	0·01280
	29·6	0·05915	0·43695	0·01129
	25·4	0·05825	0·4093	0·00977
	21·65	0·05732	0·3777	0·008175
	17·78	0·05670	0·3444	0·00674
	14·14	0·05597	0·3070	0·00527
	10·40	0·5520	0·2624	0·00381
III 54·0	45·95	0·06250	0·5417	0·01833
	44·50	0·06222	0·5317	0·01763
	42·2	0·06175	0·5207	0·01676
	40·3	0·06134	0·5101	0·01595
	36·6	0·06060	0·4869	0·01433
	29·6	0·05915	0·4359	0·01127
	21·65	0·05732	0·3770	0·00816
	14·14	0·05597	0·3069	0·00527
	6·67	0·05445	0·2090	0·00238
IV 24·85	56·61	0·0647	0·5890	0·02243
	45·95	0·0625	0·5397	0·01822
	42·2	0·06175	0·5191	0·01668
	36·6	0·06060	0·4847	0·01424
	32·9	0·05985	0·4606	0·01271
	29·6	0·5915	0·4350	0·01123
	25·4	0·05828	0·4078	0·00975
	21·65	0·05732	0·3764	0·00814
	17·78	0·05670	0·3434	0·00670
	14·14	0·05597	0·3064	0·005275
	10·4	0·05520	0·2616	0·00378

WIDE TUBES.

(1) Series and Pressure	(2) Temperature θ	(3) R_θ/l	(4) Current C	(5) $\frac{C^2R_\theta}{l}$
I 75·6	45·95	0·06250	0·5888	0·2168
	42·2	0·06175	0·5635	0·01963
	38·5	0·06100	0·5372	0·01760
	34·8	0·06022	0·5093	0·01567
	31·0	0·05945	0·4781	0·01367
	25·4	0·05825	0·4297	0·01082
	21·65	0·05732	0·3935	0·00890
	17·78	0·05670	0·3540	0·00712
	14·14	0·05597	0·3112	0·005425
	10·40	0·05520	0·2612	0·00378

The remaining readings tabulated below were taken with the bridge used for the wide tubes.

WIDE TUBES.—*Continued.*

(1) Series and Pressure	(2) Temperature θ	(3) R_θ/l	(4) Current C	(5) $\frac{C^2R_\theta}{l}$
II	39·18	0·0611	0·5440	0·01808
76·5	35·40	0·06035	0·5164	0·01598
	31·70	0·05958	0·4848	0·01402
	27·95	0·05881	0·4542	0·01214
	24·20	0·05804	0·4210	0·01030
	16·74	0·05650	0·3444	0·00670
	9·26	0·05495	0·2487	0·003395
III	39·18	0·06110	0·52415	0·01677
60·43	31·70	0·05958	0·4677	0·01303
	24·20	0·05804	0·4050	0·009532
	16·73	0·05650	0·3302	0·006160
	9·26	0·05495	0·23895	0·003160
IV	46·65	0·06261	0·5225	0·01710
32·6	42·9	0·06187	0·4993	0·01544
	39·18	0·06110	0·4768	0·01390
	35·4	0·06035	0·4530	0·01240
	31·7	0·05958	0·4280	0·01091
	27·95	0·05881	0·4020	0·00939
	24·2	0·05804	0·3724	0·00805
	20·5	0·05730	0·3426	0·00673
	16·74	0·05650	0·3105	0·005295
	13·0	0·05573	0·2732	0·004165
	9·26	0·05495	0·2323	0·002963
V	48·5	0·06305	0·5040	0·01602
11·56	46·65	0·06261	0·4943	0·01530
	42·9	0·06187	0·4752	0·01397
	39·18	0·06110	0·4558	0·01269
	35·4	0·06035	0·4356	0·01144
	31·7	0·05958	0·4138	0·01021
	27·95	0·05881	0·3886	0·00888
	24·2	0·05804	0·3638	0·00769
	20·5	0·05730	0·3365	0·00649
	13·0	0·05573	0·2708	0·004085
	5·52	0·05388	0·1806	0·00176
VI	48·5	0·06305	0·5029	0·01597
6·36	44·78	0·06225	0·4834	0·01465
	39·18	0·06110	0·4549	0·01264
	35·4	0·06035	0·4346	0·01140
	31·70	0·05958	0·4132	0·01018
	27·95	0·05881	0·3886	0·00889
	24·20	0·05804	0·3638	0·00769
	20·50	0·05730	0·3364	0·00649
	13·00	0·05573	0·2708	0·004085
	5·52	0·05388	0·1810	0·001767

In Figs. 2 and 3 the heat loss per unit length of wire in watts (column 2) is plotted against the temperature, platinum scale (column 5), for the narrow tubes at the different pressures taken. For purposes of comparison, Series I for the wide tubes was also plotted in the same diagrams. Fig. 4 gives the corresponding curves for the wide tubes.

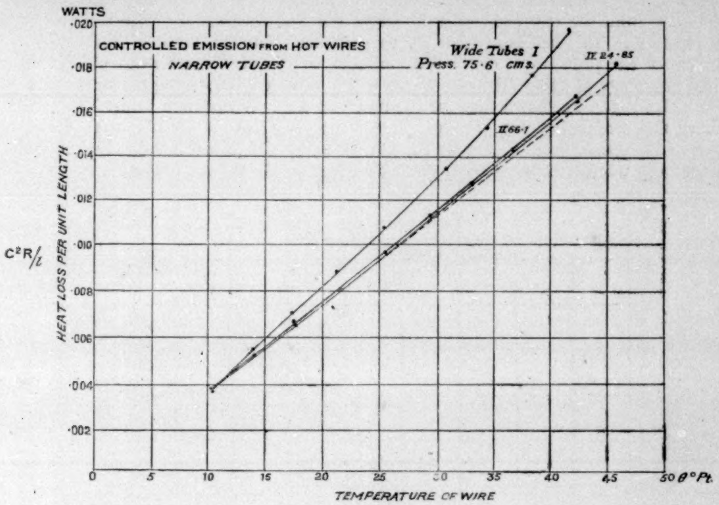

FIG. 2.

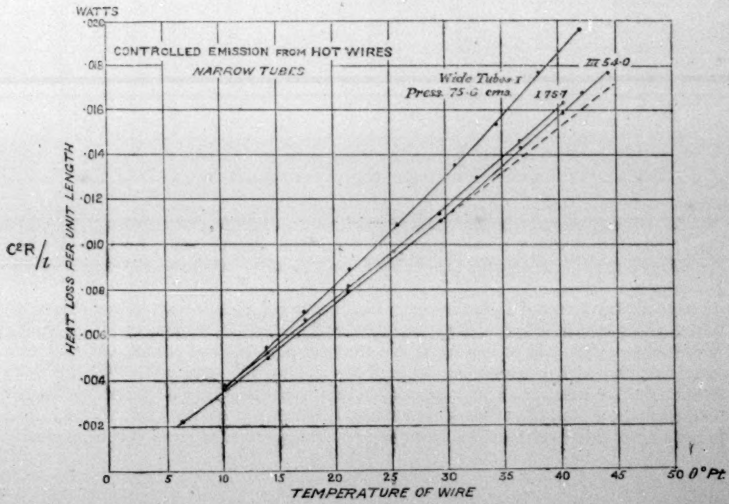

FIG. 3.

V. Discussion and Interpretation of Results.

In these results no attempt is made to separate the temperature drop across the glass from the temperature drop across the gas, between the wire and the wall of the tube. This has been done for two reasons. Firstly, because from the very nature of the problem of controlled emission of this kind there must always be a solid boundary of finite thickness across which there will be a temperature gradient. Secondly, it can be shown that for the material used (Whitefriars Flint Glass), with air as the conducting gas, the drop across the glass is only about one-thousandth part of the drop across the gas for the narrow tubes and half this value for the wide, a quantity which can have no appreciable effect on the curves obtained.

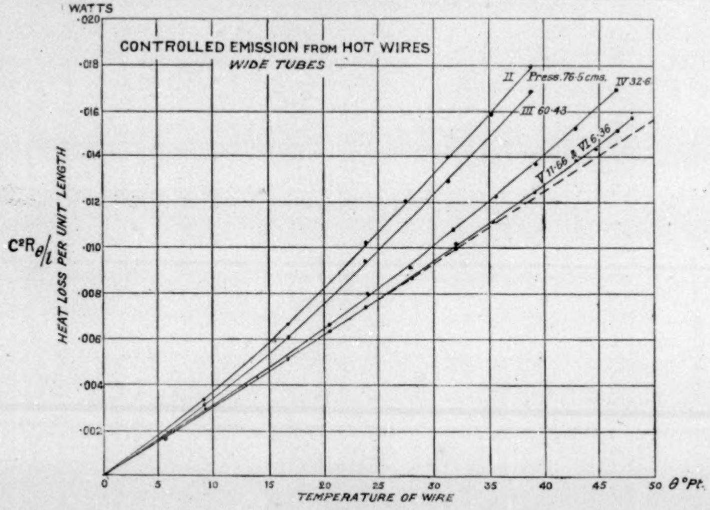

Fig. 4.

Sets of curves have been drawn showing the variation of the wattage heat loss per unit length of wire with the temperature, at constant pressure, for the wide and the narrow tubes, the dotted line being a continuation of the straight part of the low-pressure curves.

It will be observed that these curves tend to coincide for the lower pressures, but as the pressure is raised they separate and become increasingly convex to the temperature axis. The separation is due to the appearance and growth of convective losses at the higher pressures, since both conduction and radiation are here independent of pressure. The appearance and increase of convexity at the higher temperatures, which introduce a separation between the curves even at the lowest pressures, are due to convective losses and to the slight deviation of the conductivity from a linear relation with the temperature. As the lower parts of the low-pressure curves coincide, one infers that, at these temperatures and pressures, convection, being the only factor depending on the pressure, has vanished, so that under these conditions conduction and radiation alone determine the shape of the curves. It might be pointed out that for these low pressures the curves are straight lines over a fair range of temperature, showing that, the radiation losses at these temperatures being small, the conduction of heat is here practically linear with the temperature.

The results for the two sets of tubes are similar, those for the wide tubes emphasizing the above tendencies. For the narrow tubes the curves approximate to straight lines up to much higher pressures than is the case for the wide tubes, showing that the deviation from Newton's Law increases with the radius of the tube. From this it would appear that the validity of Newton's Law is contingent in a large measure upon the absence of convection.

This work as a whole indicates that the emission of heat from electrically heated wires can be controlled so as to conform with Newton's Law by minimizing the convective losses. This may be brought about by using—

(1) Suitably small temperature gradients (e.g., for tubes 1 cm. in diameter containing air at half an atmosphere pressure the temperature gradient should not exceed 20°C.).

(2) Tubes of small diameter.

(3) Pressures as low as the dimensions of the tubes will permit, remembering that the conduction is no longer independent of pressure when the mean free path of the molecules of the gas becomes comparable with the radius of the tube.

In conclusion, the authors wish to express their gratitude to Dr. Gregory and Mr. Archer for the loan of the tubes and for much valuable help and advice received during the course of the investigation.

DISCUSSION.

Dr. J. S. G. Thomas: It may be of interest to mention the results obtained by several previous experimenters relating to the loss of heat from wires, tubes, cables and plane surfaces so far as these losses are dependent on the excess of temperature θ of the surface over the atmosphere. These can be represented by empirical formulæ as follows: Peclet, $a\theta$; Porter, $a\theta$; Eason, $a\theta^{1.23}$; Goldschmidt, $a\theta$; and $a\theta^{1.25}$; Kennelly, $a\theta^{1.1}$; King, $a(1+\beta)\theta$; Ayrton and Kilgour, $a\theta$. Langmuir's results show that in some cases the heat loss is proportional to the tenth power of the temperature of the heated wire. That convection and radiation losses are not dependent in the same manner on the excess of temperature of a wire over its surroundings can be easily demonstrated by determining the form of the current-temperature curves of a wire heated in either air or carbon dioxide in a wide or narrow tube. In the case of a narrow tube the temperature of the wire corresponding to any definite value of the current is always greater when the wire is surrounded by the CO_2 than when it is surrounded by air. In this case the heat loss from the wire is mainly conditioned by loss due to radiation. In the case of a wide tube, however, in which the heat loss is due principally to convection, for small values of the heating current the wire when in CO_2 attains a higher temperature than when it is surrounded by air, whereas for large values of the heating current the reverse is the case, the wire in air attaining a higher temperature than the wire in CO_2.

Mr. Rollo Appleyard: In all the experiments described by the authors the tubes were horizontal. It has long been known that convection effects depend greatly upon the angle with the vertical at which a hot bar or wire is held. With hot-wire ammeters and voltmeters, for example, accuracy requires that the instruments shall be read only when they are at the angle at which they were calibrated. Before making general deductions from their measurements, therefore, it would be helpful if the Authors would repeat the investigation with the tubes at various angles.

Dr. **Alexander Russell**: I should like to point out that Newton only considered the problem of the convection of heat from a surface of a hot body immersed in a cooling blast of air. He considered the case of a block of iron cooled in a current of air flowing uniformly, and found that for a given wind velocity the heat carried away by the air was proportional to the difference of temperature between the body and the air before it was heated. In those days the melting point of ice, various kinds of wax, &c., gave a rough scale of thermometry. It has been verified by physicists that Newton's law is true within the limits of experimental error, even when the difference of temperature between the body and the air (before it is heated) is very great. It has nothing to do with Stefan's law, which states that the radiation of heat depends on the difference between the fourth powers of the temperature of the body and of the temperature of the cooling air, both temperatures being expressed in Kelvin's absolute scale of thermometry. Newton did not find how the amount of heat convected away varied with the velocity of the cooling draught; this was discovered by the French mathematical physicist Boussinesq at the beginning of the present century. Other things remaining the same, the rate of cooling depends on the square root of the velocity of the air; if you quadruple the velocity of the cooling blast twice as much heat will be carried away. There is a limit, therefore, beyond which it is uneconomical to increase the velocity of the blast. The rate of cooling is proportional to the temperature difference between the hot body and the cold air, as well as to the square root of the velocity of the blast, and lastly it depends on the shape of the body; Boussinesq has given exact solutions for cylinders, plates, spheres, &c. In many cases only a small error is introduced by neglecting the heat lost by radiation. It is interesting to remember that Prof. Kennelly found independently by experiment Boussinesq's solution for the convective cooling of a cylinder.

The **Chairman**, in calling upon Miss Marshall for a reply, said that it was not often that the Physical Society had the pleasure of listening to an authoress, and he felt sure those present would wish him to express the pleasure it gave them to have the opportunity of welcoming her on this occasion.

Miss **Marshall** thanked the Chairman for his kind welcome.

Dr. T. **Barratt** (communicated subsequently): Newton's law was intended to apply to losses of heat by convection. It is therefore at first sight somewhat surprising that the authors find " the emission of heat from electrically heated wires can be controlled so as to conform with Newton's law *by minimising the convection losses*. Experiments by the writer (Proc. Phys. Soc., 28, I, December, 1915) showed that the heat emission from thin wires (in a wide metal enclosure) was composed almost entirely of convective losses. It is of interest, therefore, to examine the figures given in the present Paper for indications of relative losses by convection and radiation (conduction down the wires being very neatly eliminated). Their wire had a surface area per 1 cm. length of about 0·05 sq. cm. L. V. King (Phil. Trans., 373, November, 1914) quotes a formula by Lummer and Kurlbaum for the radiation loss e from a platinum surface, $e=0{\cdot}514\ (\theta/1000)^{5.2}$ watts per sq. cm. per 1°C. excess. This gives, for the wire used by the Authors, $e=0{\cdot}0003$ watt per cm. length per 10°C. excess. Their total loss per 1 cm. length for 10°C. excess is given as 0·003 watt, so that 90 per cent. of their loss is by convection for small excesses of temperature. The authors find (as everyone does) that as their wire is raised in temperature the total loss per square centimetre increases. This is accounted for by the rapid increase in the radiation portion of the losses, which follows approximately the Fourth Power Law even for bright platinum. The authors assume that the temperature of the inner surface of their enclosure remains at 0°C. It appears doubtful, however, whether the glass would be able to carry away at a sufficiently rapid rate the amount of heat brought to it from the wire. This would be particularly evident at the higher temperatures of the wire. The authors find, moreover, that Newton's Law appears to be more nearly followed in the case of the narrower enclosures. This would naturally be so if the foregoing explanation is correct, since the narrow tube would be more strongly heated by the hot currents of air. It is even possible that a limiting diameter of tube might be found where the increase in watts per 1°C. excess would be just balanced by the increase in temperature of the glass. It would be an interesting experiment to stretch a linear platinum thermometer along and in contact with the inner surface of the glass enclosure to find whether it remains accurately at 0°C. as the temperature of the central wire is raised.

AUTHORS' reply : Dr. J. S. G. Thomas' curves for air and CO_2 we find particularly interesting, since their form is similar to our own, and also because we have recently completed an apparatus suitable for the measurement of heat transmission through CO_2. Dr. Thomas' results point to the conductivity of CO_2 being smaller than that of air, which fact agrees with the existing values for the conductivities of air and CO_2, and they also point to the convective losses being greater for CO_2 than for air. This latter conclusion we shall endeavour to verify with our apparatus.

Mr. Rollo Appleyard may be interested to hear that in our new apparatus the wires and tubes are vertical. We thank him for his suggestion of using various angles of which we shall probably avail ourselves after having examined the vertical distribution.

(To Dr. Alexander Russell.) Though Newton originally formulated his law for forced convection, several experimenters have assumed it to apply to the mixture of conduction and free convection which occurs in an originally still atmosphere. For the law to be obeyed, the relation between the total heat loss from the body and its temperature excess must be rectilinear. Our experiments were carried out in order to determine the circumstances in which this type of relation holds for an originally still atmosphere, and they have shown that this occurs only for particular values of the temperature of the wire and the pressure and extent of the surrounding medium.

(To Dr. T. Barratt.) The reply to Dr. Russell's remarks applies also to the first part of Dr. Barratt's. In an otherwise still atmosphere the heat loss is due to a mixture of radiation, conduction and convection. The conduction seems to follow a parabolic law, which, for the temperature gradients used in the foregoing experiments, is found to be practically rectilinear. This parabola is concave to the temperature axis. The curves we obtained are convex to the temperature axis where convection predominates ; therefore, by limiting convection, one can obtain a linear relation between the total heat loss and the temperature excess of the wire.

The radiation losses were deduced from the experiments of Gregory and Archer, who used the identical apparatus. Their results are in the course of publication by the Royal Society. They determined the heat loss from the wire at various temperatures *in vacuo*, and, after allowing for the "residual conductivity," they found that the radiation heat loss was a linear function of the temperature between 8°C. and 32°C. Their results show that the radiation heat loss at 32°C. is 0·0000769 watts per cm.—i.e., that it is 0·64 per cent. of the lowest value of the total heat loss at this temperature. At 8·4°C. the radiation heat loss is 0·00000925 watts per cm.—i.e., 0·31 per cent. of the total heat loss. This shows that the radiation heat loss is negligible for the scale to which the total heat loss curves have been drawn.

We have not assumed that the temperature of the inner wall of the tube is at the temperature of the bath. In Gregory and Archer's Paper on the conductivity of air it has been shown that there is a temperature gradient across the glass wall. This drop of temperature across the glass wall has been calculated from a knowledge of the conductivity of the glass used in the experiment, and, as we mentioned in the first paragraph of Section V of our Paper, we found that, for the thickness of the glass used, the temperature drop across the wall is negligible compared with that across the gas.

XXVIII.—RECENT RESEARCHES IN POSITIVE RAYS.

The Tenth Guthrie Lecture, Delivered April 24, 1925.

By Prof. W. Wien.

LADIES and Gentlemen,—I thank the Physical Society heartily for their invitation to deliver the Guthrie Lecture of the present year. I much appreciate the honour of this invitation, and hope that the progress of science, which cannot be effected by one nation alone, may increasingly be served by the co-operation of scientific men of all nationalities.

In giving you a short account of the progress of our knowledge on the subject of positive rays, I must explain at the outset that I shall not attempt to cover the whole ground. I shall leave on one side those experimental researches which are concerned with isotopes, because you in England are more familiar with that subject than we are in Germany. You will, therefore, pardon me if I confine my remarks to experiments which have been carried out in my own laboratory, and which, therefore, are more familiar to me than experiments my knowledge of which is derived from published accounts. All the admirable researches on isotopes suggested by Sir J. J. Thomson's work and carried out by Dr. Aston lie outside the scope of this lecture.

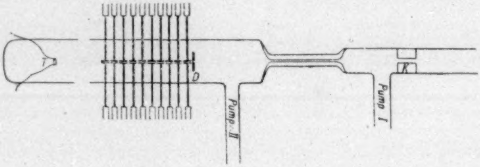

Fig. 1.

The first object of our experiments was the investigation of the conversion of charged particles into uncharged, and *vice versa*, which takes place in the positive rays owing to collisions with gas molecules. The theory of these changes is very simple. We assume that an uncharged particle moves along a certain path without any collision and then by impact with a slow-moving molecule loses an electron, becoming positively charged. On the other hand, a positively charged atom in the canal stream takes away an electron from a molecule by collision, and then goes on in an uncharged state. The mean free paths, L_1, L_2, in these two states are not of the same magnitude, the mean free path, L_2, of the uncharged particles being greater than that, L_1, of the charged. It is easy to show that the ratio of the two mean free paths is equal to the ratio n_1^0/n_2^0 of the numbers of charged and uncharged particles in the steady state.

Fig. 1 shows my first apparatus for determining the free paths and the relative numbers of charged and uncharged particles. The stream of canal rays from the perforated cathode *K*, after traversing a capillary tube and a slit *D*, passes between the plates of ten small condensers, each of which is 1 cm. wide and is separated from its neighbour by a gap of 1 mm. If one of the condensers be charged all the charged particles are deflected and only the uncharged particles pass on undisturbed. The stream impinges on a thermopile *T*, which accordingly measures its kinetic energy. If afterwards the charged particles be deflected, the energy recorded falls off accordingly. If the potential difference applied to the condenser is large enough all the charged particles are eliminated, and in that case no further change takes place in the reading of the thermopile when the potential difference across the condenser is further increased.

If we charge the first condenser sufficiently all the charged particles can be removed from the stream, but by collision with the molecules of the gas fresh charged atoms are originated. These can be eliminated by charging up a further condenser, the number of particles so charged and eliminated depending on the distance between the two charged condensers. Using only one condenser we get at once the ratio of charged to uncharged particles, and hence the ratio of the mean free paths. If we charge all ten condensers all the charged particles created throughout the path are eliminated, and the diminution of energy depends on the length of the row of condensers and on the mean free path of the particles in the uncharged state. This is the best method for determining the mean free path.

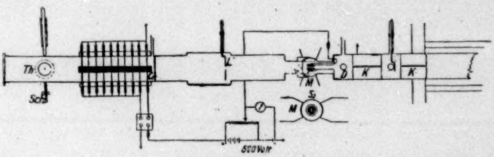

FIG. 2.

In this work I used ordinary canal rays composed of particles of mixed velocities and comprising atoms and molecules of different chemical elements. Rüchardt used a different apparatus which enabled him to study homogeneous pencils of particles, this apparatus being shown in Fig. 2. The canal rays pass through electric and magnetic fields at *M*, and the charged particles are spread out into parabolic sheets. The tube can be bent by means of a universal joint, S_2, and with the aid of a slit, *L*, in a zinc sulphide screen any desired homogeneous pencil can be selected.

Those rays of only one kind of atoms and the same velocity are going to the same system of ten condensers described in Fig. 1, where in the said manner both free paths are determined by means of a very sensible thermopile *Th*, designed by the Zeiss Works in Jena.

Rüchardt's results are very simple. We see, Fig. 3, that the reciprocal of the mean free path, L_1, when plotted against the pressure gives a straight line, but the latter does not pass through the origin as required by theory though the mean free path must be infinite for zero pressure. But Rüchardt* was able to show that this discrepancy arises from a residue of vapour in the discharge tube, which cannot be removed even by the most rapid Gaede pump. The effect could only be eliminated by using a glass vessel free from large masses of metal and from cement of any kind.

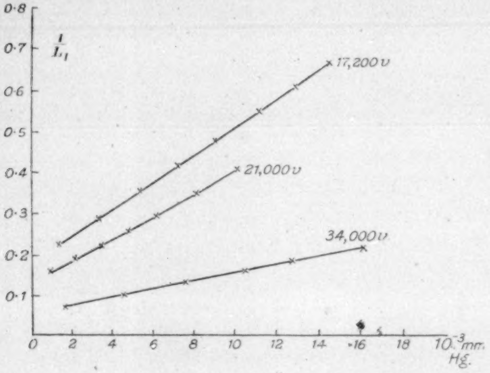

Fig. 3.

It is clear that from these substances vapour is always given off, so that a residual vapour pressure always remains where they are employed.

Fig. 4 shows the increase in the ratio of charged to uncharged atoms of hydrogen with the velocity of the particles as determined from the potential drop in the discharge tube. A potential drop of 10,000 volts gives only about 10 per cent. of charged atoms, but 35,000 volts gives more than 50 per cent. It will be remembered that the ratio of the numbers of charged and uncharged particles is also the ratio of the mean free paths. In Fig. 5 we see how the reciprocal of L_2 depends on the velo-

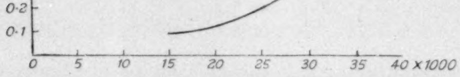

Fig. 4.

city, L_2 being the mean free path for uncharged particles. L_2 decreases as the potential is raised. L_1, on the other hand, increases as the potential is raised (Fig. 3), that is to say as the speed increases, but its rate of change with velocity is greater than that of L_2.

The kinetic theory of gases gives a simple relation between the collisional radius of an atom and the mean free path. The volume of a cylinder whose length is that of the mean free path and whose radius is twice the collisional radius of the atom is equal to the reciprocal of the number of atoms per unit volume. The collisional radius so calculated is found to lie between the first and second of the Bohr orbits.

Rüchardt's measurements are fairly accurate. We can say that we know the mean free path of a hydrogen atom moving with the velocity of the positive rays to an accuracy of a few per cent.

Besides the mean free paths for charged and uncharged atoms, we have to consider other mean paths, connected with the light emission of the positive rays. The luminosity of these high-velocity atoms also arises from collisions with molecules of the gas in the discharge tube. We have, however, two kinds of collisions to consider, viz.: (1) Those which are responsible for the emission of light, and (2) those which destroy the emission of light from an excited atom. The latter I term "perturbations." Corresponding to these we have two kinds of mean path, and there is a third kind corresponding to the duration of luminosity in a pure vacuum. Consider first the collisions which are responsible for the emission of light. If we accept Bohr's theory, we suppose that in consequence of such a collision an electron is raised to a higher energy level, where it can stay for a time which measures the "duration of luminosity." Since the minimum amount of energy emitted by an atom is $h\nu$, we can calculate the number of these light-exciting collisions and the mean path if we know the absolute value of the light energy emitted by an atom per cm. of its travel. That value I determined eighteen years ago by comparing the light emitted by the canal rays with the radiation from a black body at a temperature of about 1,100°. The intensity of this radiation being known from the laws of radiation, we can calculate the energy emitted by the canal rays, and hence also the mean path of the excited atom—that is to say, the path which begins when the atom is excited by collision, and ends when the displaced electron returns to its normal orbit with the emission of a quantum of light. We can only find this mean path

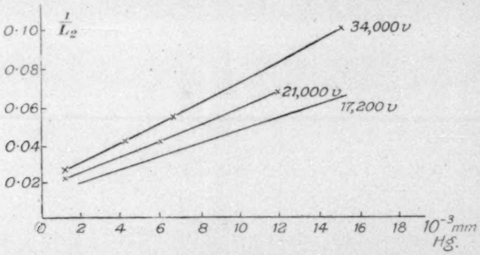

FIG. 5.

by observations in a very high vacuum, when we are sure that there are no perturbations of the light emission by collision ; for if collisions be permitted, a certain number of excited atoms will be charged by perturbing collisions before they have had time to emit light. In other words, the mean path for perturbations must be large, under the conditions of the experiment, in comparison with the mean path for the duration of luminosity. For hydrogen a vacuum of about ·001 mm. is sufficient. I was able to determine the duration of luminosity by employing an apparatus in which the canal rays passed through a short, narrow slit into a region of high vacuum. For the strongest lines of H, O, N, He and Hg, I found that the intensity of light-emission decreases according to an exponential law with distance measured along the path of the rays, and from the exponential constant we can calculate the mean luminous path. In the case of the canal rays, this amounts to a few centimetres. If, however, we observe the luminosity of canal rays at ordinary pressures, we have to take into account all the complications arising from what I have called " perturbations." Observations made at various pressures give us the mean path for

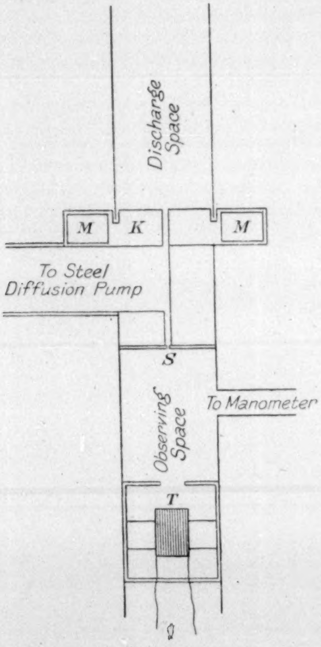

FIG. 6.

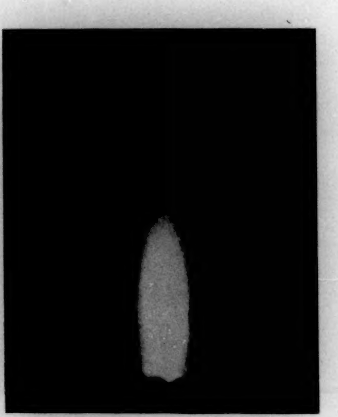

FIG. 7.

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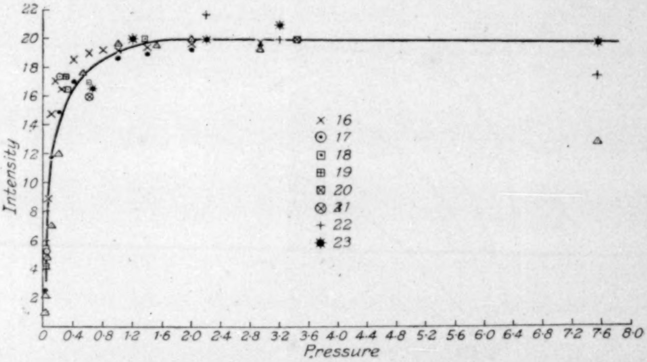
perturbations, that is to say, the path which begins when an average atom is excited by the raising of an electron to a higher energy level, and ends when the electron, by a perturbing collision, is removed from this level before the emission of light could take place. Recently I observed the luminosity of the canal rays up to a pressure of about 8 mm. of mercury. The apparatus is similar to the arrangement for the determination of the duration of luminosity, and is shown in Fig. 6. The canal rays on emerging from the slit *S* pass into the "observing space" at comparatively high pressure, while the region from which they emerge is kept at low pressure by a mercury-vapour pump. In this way it is possible to maintain a pressure of 8 mm. on one side of the slit and ·02 mm. on the other, so that the pressure on one side is 400 times that on the other.



$l=0$ $l=0$

$\lambda^{\circ}_{1}=60$. $L_{2}^{0}=300$. $\lambda^{\circ}_{1}+150$. $L_{2}^{0}=300$.

FIG. 8.

Now the intensity of the line Hα (displaced by the Doppler effect) has been compared for various pressures, and it has been found that the intensity increases in proportion to the pressure in high vacua, but for pressures of the order of ·5 mm. the intensity becomes constant. It is easy to see that this behaviour results from the influence of perturbing collisions on luminosity. If there were no perturbations, the luminous intensity should increase in proportion to the pressure, because the frequency of light-exciting collisions increases at that rate. This is, indeed, the case at low pressures, where no appreciable perturbations take place, but at higher pressures the luminosity is diminished by collisions. With increase of pressure the mean path for perturbations becomes small compared with the mean luminous path, and the increase in the number of excited atoms is compensated by the increase in the number of perturbations. Fig. 7 shows the canal rays entering a region of 8 mm. pressure and disappearing after a short distance. Fig. 8 shows how the intensity becomes constant with increasing pressure. Fig. 9 gives a comparison of experimental with theoretical curves: the agreement is quite good. In these figures L_2^0 is the mean path of the uncharged particles at a pressure of ·001 mm.; l the luminous path *in vacuo* on the hypothesis of a certain "life time" of excited atoms; λ the logarithmic decrement of the luminosity *in vacuo* in accordance with the wave theory of light; $l^0{}_1$, $\lambda^0{}_2$, are corresponding mean paths of perturbations.

It may be noted in passing that these curves answer another question. The classical theory explains the duration of luminosity by the damping down of vibrations owing to gradual loss of energy by radiation. On the other hand, the quantum theory suggests that the duration of luminosity is the statistical mean of the time which elapses between the excitation of an atom and the return of the disturbed electron to its normal orbit. It seemed not impossible that both effects might take place simultaneously. In that case we should have two kinds of perturbations, one destroying the vibrations and the other removing the displaced electron from the atom before the emission of light can be effected. But if we have two independent causes of perturbations, the intensity of luminosity will ultimately be diminished more than it can be increased by increasing the number of light exciting collisions, and the curve will drop after reaching a maximum. The observations show, however, that the intensity reaches a constant value, so that there is only one kind of perturbations. Hence we have to choose between a gradual damping of the oscillations and a statistical duration of the excited condition of the average atom.

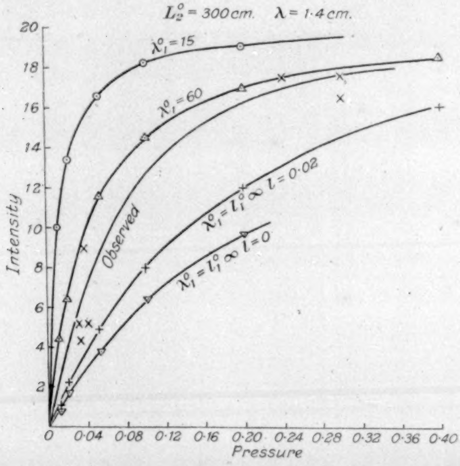


FIG. 9.

Using a modification of the apparatus designed for the measurement of the duration of luminosity, I have been able to discriminate between the spectral lines from charged and uncharged particles. The canal rays emerging from a slit into a high vacuum

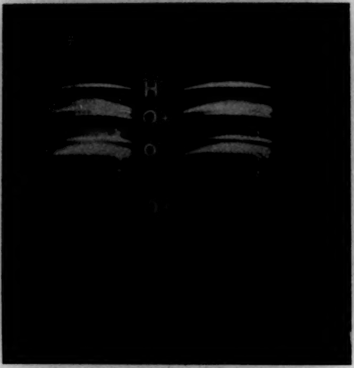


FIG. 10.

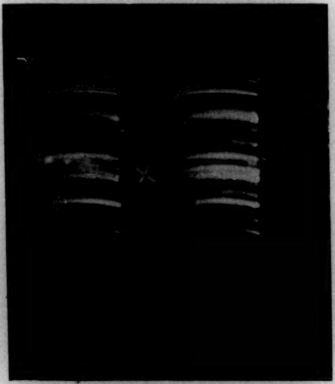


FIG. 11.

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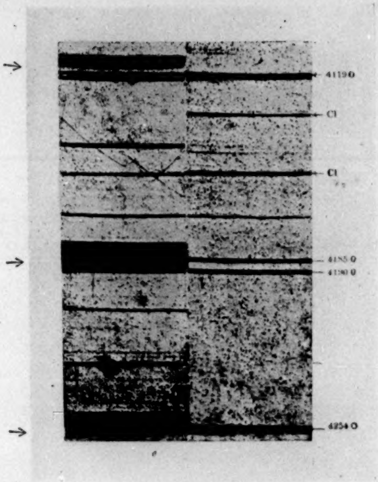
4119 O
Cl
Cl
4185 O
4190 O
4254 O

Fig. 12.

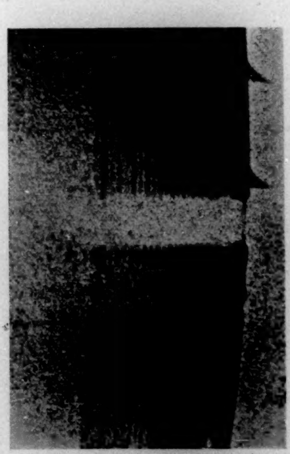

Fig. 14.

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pass between the plates of a small condenser only 1 mm. wide. The condenser must be small and very close to the slit, because in the high vacuum the luminosity of the canal rays extends over only a short distance. In the high vacuum no appreciable changes take place in the charges on the particles ; hence, if we form an image of the pencil of rays on a photographic plate by means of a spectrograph and charge the condenser, we can make out which spectral lines are emitted by charged and which by uncharged particles : in the former case the lines are deflected, and in the latter they are not. Fig. 10 shows the O and H lines. The latter are not deflected, but some of the former show a very remarkable deviation. These lines, the well-known spark lines, are emitted by charged particles. In Fig. 11 the cross marks the position of one of the nitrogen bands. Comparison of the two photographs shows that in the case where the condenser is charged the band is displaced. Some of the N bands, the " negative " bands, are emitted by positively charged molecules.

In close connexion with the problem of luminosity is the paradoxical fact that the Doppler displacement reaches a limit with increase of the voltage accelerating the canal rays. Fig. 12 shows the Doppler displacement for spark lines of O, these lines being indicated by arrows. H lines have a relatively smaller Doppler displacement. But the arc lines of O also show a much smaller displacement, and we shall find that the difference arises from the fact that the arc lines are emitted from uncharged particles and the spark lines from charged particles. Electromagnetic methods have settled beyond doubt that the velocities of the atoms increase with voltage in the manner predicted by theory, but several physicists have agreed in finding that the Doppler shift reaches a certain limit beyond which it does not increase with increasing voltage. I have suggested several researches on this question. Dr. Krefft has raised the voltage of a discharge tube filled with hydrogen to 70,000 volts, but found no limit to the Doppler shift. The maximum velocity increases according to the square root of the voltage in accordance with theory, but he finds also that the absolute value of the shift is smaller than that which would be expected from theory. This difference arises from the fact that the H lines are emitted by uncharged atoms. The Doppler displacement of arc lines measures the velocity of the uncharged atoms, while the electromagnetic method measures that of the charged atoms, and the former have on the average a much smaller velocity than the latter, so that the greater part of the light is emitted by atoms of comparatively small velocity. The spark lines being emitted from the charged atoms we find in their case that the Doppler displacement is in agreement with the results obtained by electromagnetic methods. On the other hand, the arc lines for oxygen coming from uncharged particles show a small Doppler shift like the H lines. It seems to me, therefore, that the difficulties concerning the Doppler displacement have been cleared up.

An interesting method for the observation of the Doppler effect has been worked out by Rau. His apparatus is seen in Fig. 13. Close to the path *A* of the canal rays a glass cylinder is fixed with its axis perpendicular to the rays. The rays of light coming in all directions from the canal rays are refracted by the cylinder. If the cylinder be focussed by means of a lens *L* on the slit *S* of a spectroscope, although the axial rays will not show any Doppler effect, since they are normal to the direction of motion, yet, in addition to them, rays from approaching and receding particles will be deflected into the spectroscope, so that on the spectrogram we can get spectral lines with various Doppler shifts. Fig. 14 shows a band spectrum of nitrogen. In the middle we see the cathode and images of the canal stream behind and in front of the cathode. On each side the Doppler effect is visible as a bend in the otherwise straight spectral line. Rau obtained in this way evidence of band spectra emitted by positively charged nitrogen molecules, and I was able to confirm this conclusion later by the method of deflection already explained. Rau also found by the same method, with very long exposures, that the second spectrum of hydrogen shows very faint Doppler lines. It is, however, much easier to detect curvature in a spectral line than to find the Doppler line among a great many other lines.

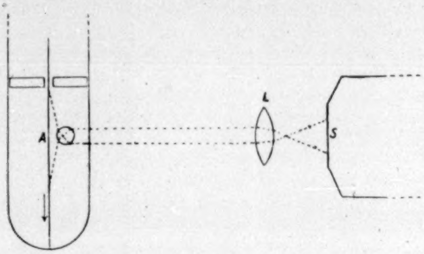

FIG. 13.

The electromagnetic analysis of canal rays using a thermopile has been further developed by Döpel. He worked with Krefft at a comparison of the velocities measured by electromagnetic deflection with those measured by the Doppler shift. Further, Döpel has compared the canal rays originated by tubes of different forms. Dr. G. P. Thomson has shown that a spherical chamber gives more H molecules in proportion to atoms than does the cylindrical type. But in spite of that the absolute number of molecules as measured with a thermopile is greater in a cylindrical tube. Fig. 15 shows how great this difference is in the case of hydrogen, *B* showing the percentage of molecules (*E*) for a spherical chamber, *A* that for a cylinder. In England the spherical chamber is preferred. Perhaps this is to be attributed to the British love of moderation.

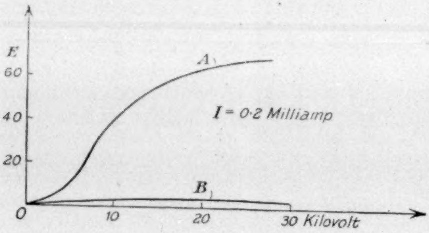

FIG. 15.

The electromagnetic analysis of positive rays by Döpel has given some interesting new results. Fig. 16 shows the parabolas obtained by means of simultaneous electric and magnetic fields. Besides the well-known parabolas of H, H_2, H_3, He, Ne, with positive charges, negatively charged hydrogen molecules and helium atoms are found. It is important to know that negative He atoms can exist.

In almost all the parabolas there are two maxima of intensity at two different velocities. Sir J. J. Thomson suggests that this is due to discontinuities in the ionisation which takes place in the discharge tube, but the parabola of H_3 shows only one maximum, and it is difficult to understand why this molecule should be the only one free from such discontinuities. The position of the second maximum suggests the explanation that it results from molecules of double mass accelerated in the discharge tube, but dissociated before entering the canal stream. We must therefore assume the existence of H_2, H_4, but not of H_6, since the parabola of H_3 does not show a second maximum. Attempts to detect H_2 and H_4 in the canal stream itself have not yet shown the supposed molecules, but I do not know what more probable explanation can be put forward for the occurrence of the two maxima.

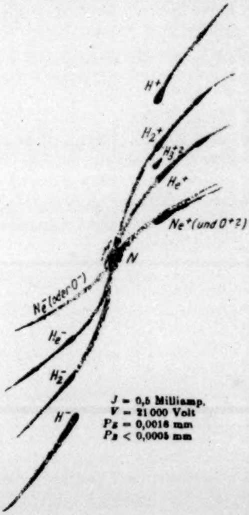

FIG. 16.

You see that the deeper we penetrate into the behaviour of canal rays the more new problems we find. The study of these rays is of great importance for physicists, because it enables us to observe the atoms under very simple conditions. But all the new questions which are raised by each new observation require the co-operation of many observers, prosecuting the work with their united efforts. British and German physicists have both contributed their share to the study of positive rays, and I hope that their co-operation will be of long duration.

XXIX.—INTENSITY OF MULTIPLE SPECTRAL LINES: EXPERIMENT AND THEORY.

Lecture delivered 22nd MAY, *1925.*

By Prof. L. S. ORNSTEIN, Rijks Universiteit, Utrecht.

THE problem of measuring the intensity of spectral lines derives its importance and sense from the theory of Bohr. In former spectral investigations attention was paid to qualitative estimation of the intensity, and spectral lines were valued without any precision. It was generally believed that the intensity of spectral lines depended haphazardly on the circumstances of emission, and therefore a precise estimation of the intensity could be considered of no use. Bearing in mind that, according to the theory of Bohr, spectral lines are emitted if an atom passes from a state of higher energy to one of lower energy, it is clear that there must be cases where the intensity of a spectral line does not depend on external circumstances; for instance, if the electron falls from an energy-level a into one of the levels b_1 or b_2. The simplest case of this would be the emission of the 4-3 and 4-2 lines of hydrogen. The first cases we worked out were all of this type, and it was only afterwards that other cases were considered and measured.

It is my intention in this Paper to outline the experimental method followed by myself and collaborators and to give a discussion of the results obtained with it. Up till now all our measurements have been performed in the photographic way; and it is only very recently that we have begun to measure spectral emission with a quick and sensitive combination of a new thermopile and a galvanometer, constructed by Dr. Moll and Dr. Burger.

If we want to compare intensities with the aid of the photographic plate, we have to measure the density caused by the light on the plate; and we have to know the relation between density and intensity. The measurement of density can be effected by different methods; in our Institute the registering microphotometer of Moll was used, which was demonstrated to this Society some years ago.

Now, if with the aid of density intensities must be compared, there is a complication that radiations differing much in wave-length have to be compared, and, as we all know, the photographic plate shows a great difference of sensitiveness for light of different wave-lengths. That means that equal quantities of energy differing in wave-length working for an equal time on the plate cause a different density.

So our experimental method had to be worked out in two stages. First we had to develop the method in the case of spectral lines of nearly equal wave-length. Later on this method was perfected by the determination of the energy-sensitiveness of the plate.

I shall begin by describing the first method in its exactest form. I shall then proceed to indicate a simplification that is possible in this case; finally, for the experimental part of this paper, I intend to describe the method used in my Institute for comparing the intensities of distant spectral regions.

If the intensity of two spectral lines of about the same wave-length has to be compared, the question can be put in this way: How much must the strongest line be reduced in order to be equal in strength to the weakest? If we have at our disposal a substance of which the absorption in the interval of wave-length used is known, we can proceed in the following way: The spectral lines to be compared are photographed with the spectrograph; and this operation is repeated, the absorbing substance being put before the slit of the spectrograph, until we get a plate where the density of the weakest line in the first photograph is the same as that of the weakened image of the strongest. It is very troublesome to proceed in this way, and it is clear that it is not the precise weakening that equalises the two lines that has to be searched for, but that a method of graphical interpolation must be used. Both spectral lines had better be photographed on the same plate during the same time, with different intensities. Instead of making the exposures one after the other, it is possible to take them at the same time. We have only to take care that the image of the spectral line on the plate is everywhere of the same intensity, and to divide this image in parts of different but known strength by putting absorbing

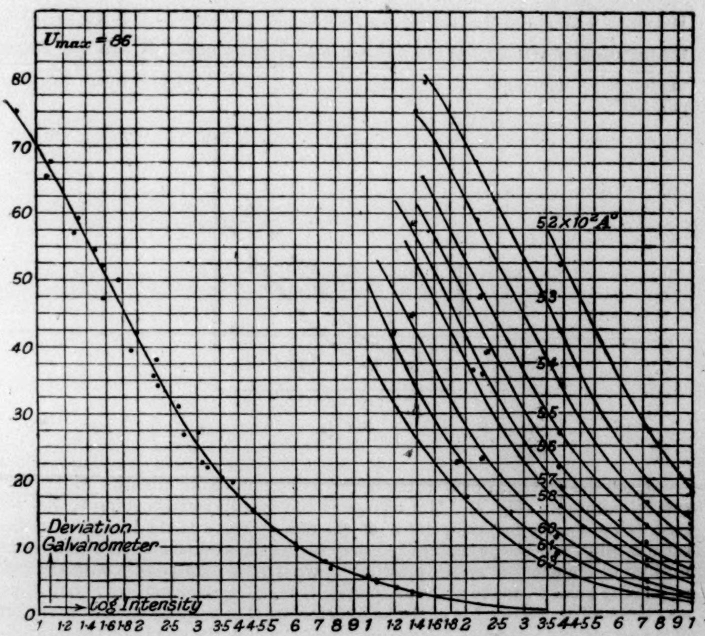

FIG. 3.

substances in the optical system. Fig. 1 shows a photograph of the triplet of Mg 5183, 5172, 5167 ; 1p—1s. Fig. 2 shows Cd 5085, 4799, 4678, 1p—1s.

By simultaneously photographing the weakened and the unweakened spectra we have the advantage that we are independent of the variations of the sources of light.

Now, from the plates obtained in this way we can easily determine the relative intensities of the lines. For this purpose they have only to be photometered, the deflection through the non-blackened plates being measured at the same time. Thereupon a graphical representation of each of the spectral lines is made by taking the logarithm of intensity as the abscissa and $\frac{U}{U_0}$ as ordinate. In this way we get the following representation, where for every line the unweakened image has been put 100.

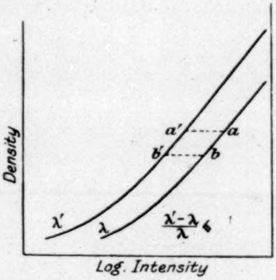

FIG. 4.

From this graphical representation the proportion of the intensity of the lines can be easily seen (Fig. 4). If *a* represents the density corresponding to 100 per cent. of the second line, the point *a'* on the density curve of the first line has the same density, and the percentage intensity of the second line may immediately be read off as the abscissa of the point *a*. If we compare the point *b* with the corresponding point *b'*, we know what fraction *b'* is of the strongest line ; and as we also know what fraction *b* is of the weakest line we thus get a second measurement of the proportion. As is easily to be seen, the distance of the graphs on the logarithmic scale in both cases yields the proportion of the lines—i.e., the density curves are parallel. Better than by repeated measurements, the proportion of the lines can be determined by applying the parallelism of the density curves. This parallelism can be used in order to draw the curves as exactly as possible, and then to determine the distance with the greatest precision possible. In this connexion the remark may be important that, not only for narrow intervals of wave-lengths the parallelism can exist (compare Fig. 3) ; to a high degree it depends on the kind of plate and the mode of development used.

Before leaving the subject of this method of measurement, I should like to say a few words about the method of weakening used. Whereas for absorption-spectra

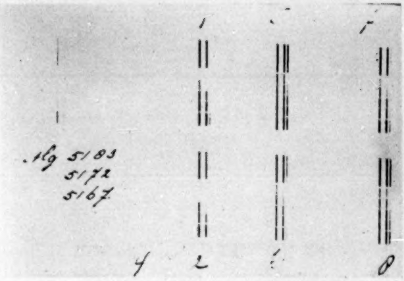

FIG. 1.

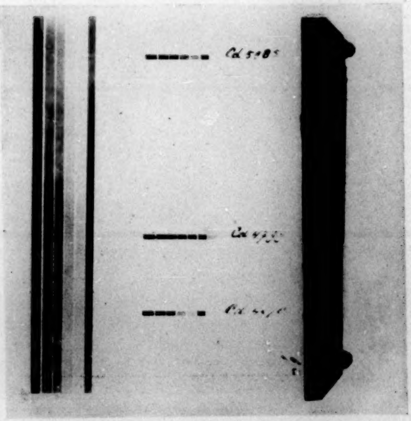

FIG. 2.

To face page **336.**]

the marks of density can be given by an echelon-form slit, or by any other form of a geometrical weakening of measurable diaphragms, this mode of operation is not practicable for line-spectra. In our researches we have made use of smoke-glass weakeners, adjusted on a lens, Le_1, which is homogeneously filled by the source of light. A second lens, Le_2, forms a sharp image of the weakeners on the slit Sp of the spectrograph.

In this way the plates demonstrated (Figs. 1 and 2) have been photographed. This method requires an astigmatic spectrograph. The demonstrated plates have been taken with a grating arrangement described by Van Cittert.

Now the smoke glass has to be standardised as to the fraction of light absorbed. This can be effected in two ways—

1. In the arrangement with a continuous source of light. The marks of intensity are given by variation of the width of the slit, the intensity for each wave-length being proportional to the width. In the way described above the density caused by the source of light when the weakeners are interposed gives a measure for the absorption.

2. The smoke glass can be standardised by direct measuring with thermopile and galvanometer, the weakeners being interposed in the beam of light produced with the help of a monochromator. This second method is simpler but less certain for those cases where the loss of light is partially due to scattering or other causes besides absorption.

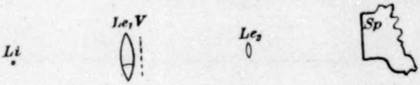

FIG. 5.

In the ultra-violet, Dorgelo (Philips) and Frerichs (Bonn) have used other methods of weakening, smoke glass not being applicable in this region.

Of late, investigations made by Dr. Burger and myself have shown that the whole method may be considerably simplified. In the beginning many precautions were necessary on account of the bad reputation of photographic methods as to precision. However, as on closer examination this bad reputation appeared to be undeserved, and rather exact results were to be obtained by a careful handling of the method, it became possible to proceed in a less complicated way. We were able to give the marks of density independently with another source of light, and it appeared to be unnecessary to take equal times of exposure for the spectrum under consideration and the density marks. In this way we have investigated the spark-spectrum of Al, Zn, Si, exposing the photographic plate for one or two seconds, and putting on the same plate, during 30 seconds, the continuous spectrum of an incandescent lamp with silicon window. This lamp yields sufficient intensity in the far ultra-violet for producing marks of density by variation of the width of the slit. In proceeding in this manner we found that measurement on multiple lines can be performed as exactly as was possible in the more complicated way described above. The proportion of the spectral lines appeared to be as well in accordance with the predictions as were the results for the visible part of the spectrum. The density curves are exactly parallel for the plates used, even for those wave-lengths which are at a great distance form each other. Complications were only to be found in the region of gelatine absorption. In this connexion we may mention that also for the X-ray region the photographic method may be applied, as is demonstrated by Dr. Bouwers in his Utrecht dissertation.

An interesting feature, albeit of no great use for the application, is the fact that in the visible as well as in the Röntgen region the density for plates developed without KBr may be represented by the formula

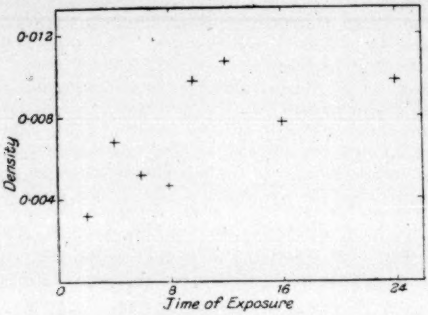

FIG. 6.

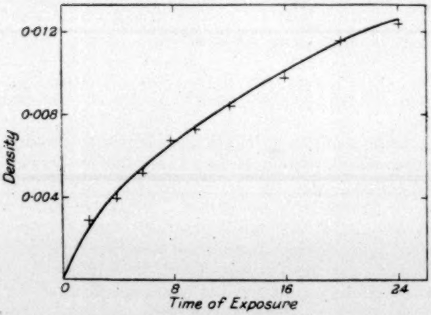

FIG. 7.

$$d=\log_{10}\frac{U_0}{U}=c\log.\left(\frac{it^p}{a}+1\right)$$

where c, p and a are constants, t is the time of exposure, and i the intensity of the light falling on the plate. Figs. 6 and 7 represent the density curves before and after a correction for the faults of the photographic plate has been applied to them.

Hitherto we have confined ourselves to the measurement of intensity for those cases where the spectral sensitiveness of the photographic plate—i.e., the density caused by equal energy in equal time—is the same for different wave-lengths. It is a well-known fact this is no longer the case if the difference of wave-length is greater.

Fig. 8 indicates the density caused in equal times on a panchromatic plate by the energy of an incandescent lamp, the energy curve being given in the same diagram. The figure shows that the density is a very bad measure for energy, and that, therefore, the qualitative measuring of density in the spectrum has hardly any significance.

In order to compare the energy of more distant spectral lines, we have used a standardised lamp. As such we chose a common 4 volt nitra incandescent lamp

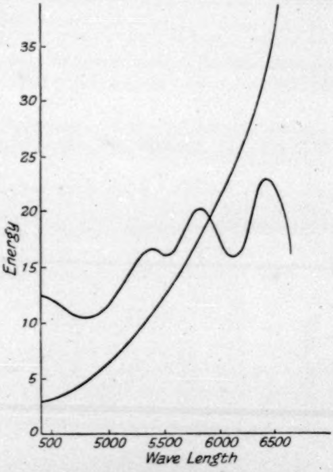

FIG. 8.

considerably under-run (2·5 v.), in order to obtain a longer duration of life. I will briefly describe the method of standardising the lamp. A monochromator has been used that was built up out of two identical spectroscopes in such a way that the spectrum formed by the first was folded together by the second on its own slit. Now, if we put a slit in the spectrum of the first spectroscope only that wave-length passes the instrument which falls on the intermediate slit.

The monochromator constructed by Dr. van Cittert excels by its strength of light and spectral purity. Now, in order to standardise a lamp with this monochromator we proceed as follows :—

Before the monochromator a strongly over-run lamp is placed, and by lenses an image of the lamp is formed in the point T. For the different colours corresponding to the different positions of the slit in the monochromator the intensity of this image is measured by a thermopile. Thereupon the slit in the monochromator is removed, which causes a white image at the point *P* of which we know the spectral composition. By means of a lens, L_2, an image is formed of *P* on the slit of a spectrograph. Then at the point *P* the spiral of the standard lamp is placed, and again a photograph is taken. The strongest of the two images on the slit is weakened in known proportion in order to get marks of density for comparison of the spectra of both lamps.

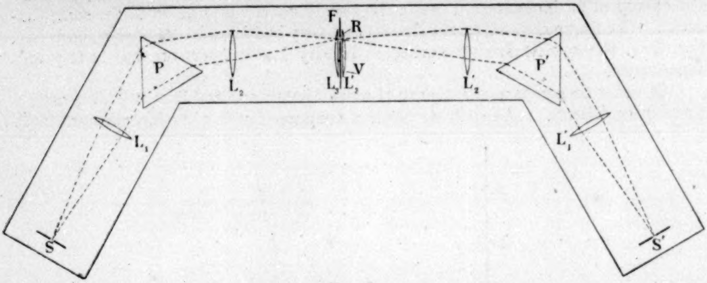

FIG. 9.

This comparison can be made for every wave-length. And, as in the image of the over-run lamp, the proportions of the energies are known, those of the unknown lamp can in this way also be determined. Fig. 8 shows the emission curve of a standardised lamp.

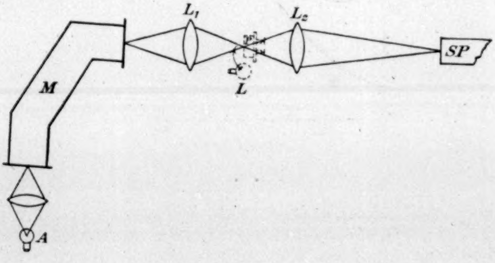

FIG. 10.

The lamp thus standardised may be used for the comparison of distant spectral lines. Fig. 2 shows the Cd triplet with, on the same photographic plate, an image of the standardised lamp.

Now the density curves for the lines as well as for the lamp are constructed. For each wave-length the proportion of the spectral line and the continuous spectrum are determined from the distance of the density curves. And as for the continuous spectrum, the proportion of the intensity for two wave-lengths is known, the proportion of the lines can be determined by this method. The standardised spectrum helps to determine the curve of sensitiveness for the plate.

The above explanations may suffice for a description of our methods. In the following paragraphs I may be permitted to try to give you a short sketch of the results obtained.

As I have mentioned in the beginning, if the intensity is an atomic quality, just like the frequency, then we may expect some simple relationship for the intensity of those spectral lines which are emitted in cases where the electron moves from one energy level to two or more other ones. Such cases occur in the emission of the sharp subordinate series, where the electron falls from the simple *s* to the multiple *p* level.

Sommerfeld and Landé have characterised these levels by whole numbers, the so-called internal quantum numbers (j) which are related with the internal magnetic field of the atom. From these internal quantum numbers the statistical weight G of the level can be determined. This weight is proportional to the number of the possible orientations of the path of the electron relative to the trunk of the atom.

Now, from the measurements in the sharp subordinate series there appeared a most remarkable result. For the doublets

Na	1p—3s (6160–6154)	1p—4s (5153–5149)
Ka	1p—4s (5802–5182)	1p—5s (5339–5323)
Rb	1p—4s (6160–6071)	
Cs	1p—5s (6034–5839)	

the proportion of the components appeared to be 2 : 1, with an experimental error of less than 4 per cent.

For the triplets of the earth-alkalis this proportion was found to be 5 : 3 : 1. This result was obtained in my Institute, first for arc-spectra by Dorgelo and afterwards confirmed for the flame spectra of alkalis by Miss Bleeker and Mr. Bongers. Prof. Sommerfeld drew my attention to the fact that the proportion of the intensities for alkalis and earth-alkalis is equal to the proportion of the statistical weight of the final levels. Therefore the triplets of the sharp series of higher multiplicity were examined. The prediction for the intensities was for

quartet system	6 : 4 : 3
quintet	7 : 5 : 3
sextet	8 : 6 : 4
septet	9 : 7 : 5
octet	10 : 8 : 6

The measurement teaches us that, for the triplet of the sextet and the octet system of manganese, the predicted proportion holds true. For Mn λ (6021, 6016, 6013) the ratio found by Dorgelo is 100 : 77 : 53, the ratio expected 100 : 75 : 50 (sextet system); and for λ (4823, 4783, 4754) 100 : 80 : 61 found, and expected 100 : 80 : 60.

Meanwhile, we found that even in the cases where a priori the proportion might possibly depend on the conditions of excitation—i.e., for the cases of the diffuse subordinate series—and also for some cases of the principal series where there is no self-reversion, the proportions do not depend on the circumstances, and may be described by relatively simple whole numbers. Thus there appeared to exist in those problems of intensity a much profounder regularity than there had even been expected. I will formulate this regularity in the form given to it by my cooperator Dr. Burger, in connexion with the experimental results of Dorgelo described above.

In order to formulate those rules we consider the *p–d* composed doublet of the alkalis.

	G	Intensities.	
40	4	4	36
20	2	20	0
10		4	6
	6	24	36

In multiplying the sum of the statistical weight of the initial levels (6) with the statistical weights of the final levels, we obtain numbers which are proportional to the energy radiated towards these levels. On the contrary, we obtain numbers proportional to the intensities of the lines starting from the respective initial levels by multiplying the sum of the statistical weights (10) of the final levels with the respective statistical weights of the appropriate initial levels. The diagram gives the calculation for the *p–d* case (diffuse subordinate series of the alkalis). In the case of Na, Ka, Rb the *d*-level is not split up. The intensity 36+4 is observed as a whole. In the diffuse series of all those alkalis we have found the proportion of intensities 2 : 1. In the case of Cs, the *d*-level is sufficiently split up, and we found in accordance with the rule a proportion 9 : 1 : 5. The sum rule has been confirmed by a number of further measurements which were conducted as well in our Institute as by Dr. Dorgelo (at present at Philips Lamp Works Laboratory), and by Dr. Frerichs in the Bonn Institute. The sum rule does not only hold for the spectra of atoms, but also for the ionised atoms in the spark spectra, as recent researches of Dr. H. C. Burger and myself have shown. The same is true for the doublets in a quartet system.

It is worth mentioning that recent measurements in my Institute have shown that for several lines of the principal series of Rb and Cs the ratio of the doublet is 2 : 1, contrary to the older results of Füchtbauer, who measured the ratio of absorption coefficients. The sum rule does not generally suffice to determine all intensities. If we take, for instance, a triplet case, three of the intensities are fixed by the rule that those intensities are zero for which the statistical weight would change with another value than 0 or 2. In the triplet scheme the intensities corresponding to the transitions 5-7 and 1-3 can be fixed directly. Now, if we put the transition 5-3=x, the other intensities are fixed by the sum rule, whereas the value of x is left undetermined by this rule

	G	Intensities.		
75	5	x	$12-x$	63
45	3	$12-x$	$33+x$	0
15	7	15	0	0
15		3	5	7
	g	27	45	63

In the same way we may proceed for higher multiplicities. The indefiniteness, however, grows. So far as measurements have been performed the sum rule was always confirmed.

If we take a more general triplet, we obtain for the azimuthal quantum number *k* the following table.

G	Intensities.		
$2k+1$	x	$3\cdot 4-x$	$3(4k^2+4k-3)$
$2k-1$	$3\cdot 4-x$	$3(4k^2-5)+x$	0
$2k-3$	$3(4k^2-4k-3)$	0	0
	$2k-1$	$2k+1$	$2k+3$

It would be easy to give more schemes of this type ; however, this one may suffice to show that the proportion of the terms of the diagonal to the terms of the rows parallel to it is for large values of K :

$$K^2 : \text{Const} : \frac{1}{K^2}$$

If we put, with Sommerfeld, $j_a=k-1$, and if j_s[*] represents the internal quantum number of the *s* level in the multiplet system, then the constant part of the row next to the diagonal can be represented by

$$2r\{j_s(j_s+1)-(j_e-j_a)(j_a-j+1)\}\ ,\ r=2j_s+1$$

I must draw attention to the fact that considerations developed by Sommerfeld and Heisenberg on the basis of an atomic model and of the Bohr correspondence principle, had led to results which are analogous to those obtained from the sum rule. Therefore Dr. Burger and I have tried to generalise the old Sommerfeld-Heisenberg theory of intensities towards the cases mentioned above. On the basis of our work, Kronig, Sommerfeld and Höhnerl have given other generalisations. However, all these generalisations fail to describe the intensities observed, as measurements on a multiplet of Fe performed in my Institute have shown, so that this part of the problem is still open.

Besides in the intensities in series we have also worked on the intensity problem of the Zeeman effect. It is possible to predict the intensities of the simplest Zeeman effects by simply extending the rules found for the multiplets. Kronig and Höhnerl have generalised our considerations for more complicated cases of the Zeeman effect. Whereas our measurements of our own plates and of plates kindly sent us by Dr. Back in Tübingen confirm the theory. I will describe here our considerations for the *p–s* triplet.

The splitting up is determined by the so-called magnetic quantum numbers, indicated in the first row. The letters indicate the intensities. $\parallel$ or $\perp$ means the polarisation in the transversal effect. Now we have introduced the following hypotheses :—

1. Every line as a whole is unpolarised.
2. The total energy radiated from the magnetic level to 2, 1, 0, —1, —2, is equal for each of the lines.

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[*] **In the notation of Sommerfeld, $s=0$ for the singlet, $\frac{1}{2}$ for the doublet, 1 for the triplet system, and so on.**

3. The total energy that reaches the level 1, 0, —1, is equal for each line.

4. In order to determine the relative intensity of the components of all the lines, we use the proportions following from the multiplet theory.

		p_1	s	p_2	s	p_3	s
2	1	a_1					
1	1		C_1		y_1		
1	0	a_2		x_2			
0	1	a_3		x_3		α_3	
0	0		C_2		y_2		β_2
0	—1	a_3		x_3		α_3	
—1	0	a_2		x_2			
—1	—1		C_1		y_1		
—2	—1	a_1					
		⊥	‖	⊥	‖	⊥	‖

The figures *a*, &c., represent the intensities of the ⊥ and the ‖ components. The given rules lead to

$$a_1+a_2+a_3=C_1+\frac{C_2}{2}=5p \qquad a_1=3p \quad a_2=\frac{3}{2}p \quad a_3=\frac{p}{2}$$

$$a_1=\frac{C_1}{2}+a_2=2a_3+\frac{C_2}{2} \qquad C_1=3p \quad C_2=4p$$

$$a_1+\frac{C_1}{2}+a_3=2a_2+\frac{C_2}{2}$$

$$x_2+x_3=y_1+\frac{y_2}{2}=3p \qquad x_3=x_2=\frac{3}{2}p$$

$$x_2+\frac{y_1}{2}=2x_3+\frac{y_2}{2} \qquad y_2=0$$

$$\frac{y_1}{2}+x_3=2x_2+\frac{y_2}{2} \qquad y_1=3p$$

$$\alpha_3=\frac{\beta_2}{2}=p$$

The experiments have confirmed our calculations.

Besides the theoretical meaning of the sum rule in relation to the correspondence principle, there is still another way to make its contents clear. Let us take, for instance, the case where the radiation is produced by an electron falling from the two levels p_1 and p_2 on one *s* level. Now the intensity is proportional to the number of atoms at the level, and to a probability of transition. It may, therefore, be represented by

$$N_1 A_{p_1 s} \text{ resp. } N_2 A_{p_2 s}$$

or

$$\frac{N_1 A_{p_1 s}}{4} = \frac{N_2 A_{p_2 s}}{2}$$

Now N_1 is proportional to the statistical weight G_1 and to a Maxwell-Boltzmann function $e^{-\varepsilon_1/kT}$, where ε_1 represents the energy of the level s. In the same way N_2 is proportional to $G_2e^{-\varepsilon_2/kT}$. As a rule, $\varepsilon_1-\varepsilon_2$ is small, so that

$$\frac{G_1A_{p_1s}}{4}=\frac{G_2A_{p_2s}}{2}$$

or

$$A_{p_1s}=A_{p_2s}$$

which holds strictly for infinite temperature. The sum rule, therefore, takes this form : *the probability of transition is equal for both p levels.*

Let us now consider the case that the radiation takes place from one level to two others, with the statistical weights 4 and 2, and therefore with a proportion of the intensities 4 : 2.

If A_{sp_1} and A_{sp_2} indicate the transition probabilities, we have the equation

$$\frac{A_{sp_1}}{4}=\frac{A_{sp_2}}{2}$$

However, we will indicate another simpler relation that exists for this case. Einstein in his theory of radiation has introduced the probability of absorption for a unit of radiation density in addition to the probabilities of transition. He calls them

$$B_{p_1s} \text{ and } B_{p_2s}$$

Einstein has proved that the existence of the Planck radiation formula requires

$$G_sA_{sp_1}=\frac{8\pi}{c^3}h\nu^3{}_{sp_1}G_1B_{p_1s}$$

and

$$G_sA_{sp_2}=\frac{8\pi}{c^3}h\nu^3{}_{sp_2}G_2B_{p_2s}$$

where c is the velocity of light, h Planck's quantum and ν_{sp_1}, &c., the frequencies of the lines considered. From this we get

$$\frac{A_{sp_1}}{A_{sp_2}}=\frac{G_1}{G_2}\cdot\frac{B_{p_1s}\nu^3{}_{sp_1}}{B_{p_2s}\nu^3{}_{sp_2}}$$

or with the relation for $\frac{A_{sp_1}}{A_{sp_2}}=\frac{4}{2}$

$$\nu^3{}_{p_1s}B_{p_1s}=\nu^3{}_{p_2s}B_{p_2s}$$

For a more complicated case we find

$$G_7A_{75}+G_5A_{55}+G_3A_{35}:$$

$$G_5A_{53}+G_3A_{33}:$$

$$G_3A_{31}::$$

$$G_5:G_3:G_1$$

Now we have by the Einstein relation

$$G_7 A_{75} = \frac{8\pi}{c^3} \nu^3{}_{75} B_{75} G_5$$

therefore

$$\nu^3{}_{57} B_{57} + \nu^3{}_{55} B_{55} + \nu^3{}_{35} B_{53} = \nu^3{}_{53} B_{35} + \nu^3{}_{33} B_{33} = \nu^3{}_{31} B_{13}$$

and also

$$G_7 A_{75} : G_5(A_{55} + A_{53}) : G_3(A_{35} + A_{33} + A_{31}) :: G_7 : G_5 : G_3$$

or

$$A_{75} = A_{55} + A_{53} = A_{35} + A_{33} + A_{31}$$

which can be generalised easily.

It may be of great importance to remark that the quantity $\nu^3{}_{p_1 s} B_{p_1 s}$ can be reduced to a universal quantity if we adopt the light quantum hypothesis. Einstein assumes that for a radiation density the probability of absorption can be represented by $B\rho$. If we postulate light to consist of quanta $h\nu$, then $\rho = nh\nu$, where n is the number of quanta per unit of volume. Burger and I have proved that the probability for a quantum to strike an atom is proportional to λ^2, where λ represents the wave-length. The number of quanta that strike an atom per unit of time, and that are absorbed, can be represented, therefore, by $C\lambda^2 n$, where C is a constant depending on the character of the atom.

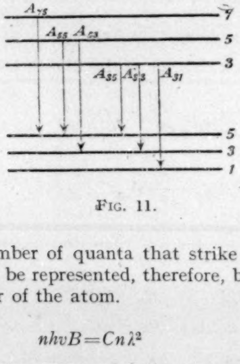

FIG. 11.

Now we have

$$nhvB = Cn\lambda^2$$

or

$$Bv^3 = C$$

Therefore we obtain the rule that the sum of the absorption probabilities per energy-quantum is the same for all the levels of a multiplet.

The fact that there exist regularities of intensity to such a degree makes it desirable to extend research to other cases also. In the Röntgen region similar rules have been established.

At present I may mention some other problems which have been partly solved. It will be known that in many cases the intensity stongly depends on the conditions of excitation. For instance, if electrons of given velocities are sent into Na vapour, it will be possible to excite the *D* lines separately. Still, it is possible to define a spectrum that is normal as to the intensities, this being the spectrum emitted at very high temperature. At such a temperature every state of the atom will be present proportional to the statistical weight, and it will emit proportionally to its inherent probability. To approximate this spectrum theoretically as well as experimentally will be the intensity problem of the future.

There are some experiments which perhaps show the existence of such a spectrum. They relate to observations of the intensity in flames and the arc of the subordinate series of the alkalis Na, Ka, Rb, Cs. From the work of Miss Bleeker and Mr. Bongers it appears that the intensity of the terms of the series as a function of the ordinal number is independent of temperature, the temperature having only a secondary influence on the total intensity of the flame. A discussion of these results will appear shortly on the base of the correspondence principle. In a flame the atoms are ionised by a chemical process, and then send out the normal spectrum.

Finally, I should like to mention the possibility of determining the energy of the levels by means of intensity measurement. The experiments have not yet been finished, but the provisional results already show this possibility.

We have determined the change of several spectral lines of the mercury arc as a function of the energy put into the arc. Lines emitted by the electron when starting from levels for which the energies differ but little have the same dependance on the energy put into the arc. If sufficient material be obtained, the slopes of the curves energy put in the arc energy of the level, expressed as a function of the latter, will allow us to calculate the energy of unknown levels.

I hope that my lecture has contributed to showing the importance of quantitative measurement of intensities, and that I may have succeeded in pointing out to you that the intensity of spectral lines represents an atómic quality as important for our knowledge of the atom as are the frequencies of radiation of their Zeeman-effect.

Permit me, in conclusion, to thank you most cordially for the opportunity you have given me to expound these problems to your Society.

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DEMONSTRATION OF SOME SIMPLE APPARATUS FOR THE ESTIMATION OF CARBON DIOXIDE.

By Dr. Ezer Griffiths, of the National Physical Laboratory.

APPLES respire, generating carbon dioxide. It has been found that the atmosphere of a gas-tight chamber gradually becomes richer in carbon dioxide and deficient in oxygen, and when the carbon-dioxide content exceeds a certain value the apples are liable to become diseased. The instruments demonstrated were devised so as to provide marine engineers with simple means of making analyses of the atmosphere in the ship's holds carrying an apple cargo.

One was of the simple gas burette type, in which a measured volume of the gas under test is displaced into a potash bulb and then returned to the burette for measurement after absorption of the carbon dioxide. Float valves are provided to prevent potash solution being accidentally drawn back into the burette or water swept over into the potash.

The novel feature of the second was a large hollow-barrel glass tap. Absorbent for carbon dioxide was packed into this hollow barrel. In one position of the tap a sample of gas was pumped through into a tube ; in the second position the absorbent was in communication with the sample of gas resulting in an absorption of the carbon dioxide and consequent diminution of volume of the sample which was measured.

The third instrument was a large-bore tube bent into a nearly complete circle. This tube was mounted on an axis through the centre of the circle, so that it could be partially rotated. A large drop of mercury functioned as a piston for displacing the sample of gas contained in the tube through an absorbent cartridge. The diminution in pressure was indicated on a dial gauge calibrated to read in carbon-dioxide percentages.

The fourth instrument was a specially-designed brass pump, by means of which a sample of gas is withdrawn from the ship's hold, and then displaced through an absorbent cartridge. As in the previous instrument, the carbon-dioxide content is indicated on a dial gauge.

DEMONSTRATION OF SOME EXPERIMENTS TO ILLUSTRATE THE APPLICATION OF THE SHAKESPEARE KATHAROMETER TO PHYSICAL RESEARCH.

By Dr. H. L. Daynes, of the Cambridge Scientific Instrument Co.

THE following characteristics of the instrument were pointed out :—

(1) A direct reading is given without the withdrawal of any gas for analysis or disturbance of the conditions of the experiment.

(2) The readings are very rapid. The half-value-period for hydrogen-air mixtures is of the order of 10 seconds, and is longer for gases of higher molecular weight. The actual relation between the periods for different gases is calculable from the diffusion constant for the mixture considered.

(3) The capacity is small, being only about 0·5 c.c. Small quantities of gas, therefore, give large changes of percentage in the mixture if the system to which the instrument is connected is also small.

(4) The instrument is sensitive to small changes of composition. With a reflecting galvanometer a charge of one part of hydrogen in 100,000 of air can be detected if the change takes place not so slowly that zero changes mask the deflection.

(5) The instrument is essentially a differential one, and has all the advantages of the differential method in eliminating the effects of variations other than the particular one to be studied. The full sensitivity of the method may, therefore, be utilised in a great variety of experiments.

In illustration of the above points, the following series of experiments was shown :—

(1) Measurement of a small rate of flow of gas by addition of hydrogen at a known rate from an electrolytic cell, and measurement of the resulting percentage of hydrogen.

(2) The rise in CO_2 percentage in an enclosed space due to the respiration of a small bee. A few thousandths of a cubic centimetre were detectable.

(3) The leakage of CO_2 and H_2 from a few square millimetres of two rubber balloons was visible in a few seconds.

(4) A method of demonstrating the effect of " thermal diffusion " in producing separation of hydrogen from oxygen. The method gives rapid results, which can easily be interpreted in absolute units to give values of the " Chapman " constant for a mixture. A simple method of calibrating is included in the apparatus.

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